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Memoir upon the Stability of Revetments, and of their Foundations. *By* M. PONCELET, *Chef de Bataillon du Génie.* *Translated from "No.* 13 *du Mémorial de l' Officier du Génie," by Captain* JOHN SANDERS, *Corps of Engineers.*

[CONTINUED FROM PAGE 295.]

CHAP. III.—*Transformation of Profiles, and relative Stability of Revetments.*

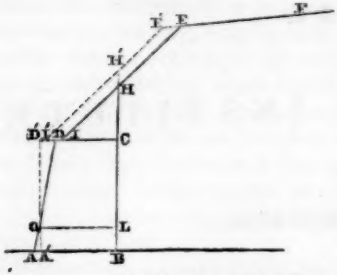
The question of the transformation of profiles, considered under its most general aspect, is of very great utility, and often presents itself in cases arising in practice, particularly when it is desired to construct, under distinct conditions, a profile of a revetment after an existing model, and which offers the same degree of stability. We propose to enter into some developments in regard to it, though confining ourselves to the usual case of the question.

Rules for the Transformation of Profiles with outer face sloping.

67. Ordinarily, we only consider the proper stability of a revetment, without regard to the changes which may take place in the action of the earth. The question of the transformation of a profile, with or without counterforts, then reduces itself to a simple question of statics, in which we propose to give to a new wall, a moment, or a friction upon its base, equal to that of an existing wall. However, if the case is restricted to revetments with the inner faces vertical, and without counterforts, it is easy to perceive the want of exactness in

this manner of proceeding, especially when it is applied to demi-revetments.

Fig. 4.



Let there be, for example, a profile ABCD (fig. 4), with its outer face in batter, and another, A'BCD', a transformed vertical one of an equal stability, or the moment of which, with reference to the outer edge passing through A', is equal to that of the preceding one with reference to the point A. It is clear, unless the exterior slope of the parapet retains about the same position with reference to BC, the inner face of the wall; or, which comes to the same thing, unless D'I, the berm of the new wall, equals DI, that of the old wall, plus D'D; that the thrust, or pressure, against this inner face, as well as the load of earth on the coping of the wall, will be modified in a manner so much the more perceptible, as the quantity D'D, and the common height, BC, of the two walls, become greater. Notwithstanding the opposition of the effects due to these two causes, we have seen (21 and 35) that they can, nevertheless, give rise to very appreciable differences in the thicknesses, or moments; which differences will sometimes be positive, and sometimes negative, according to the value of a, the ratio of the height of the superincumbent load of earth, to that of the wall.

The same observation being applicable to the case of sliding, in which the existence of a berm has for an effect, to diminish the thicknesses (see table number 53); therefore, in the present question, it is necessary, for exactness, to suppose such a width to the berm of the transformed profile, that its inner face may nearly preserve the same situation with reference to the mass of the embankment. We say *nearly*, and not exactly; because the outer edge, A, or A', in fact, experiences, with regard to the inner face in question, a displacement AA', which, in case of rotation around this same edge, tends to modify the length of the arm of the lever of resistance, or stability, due to the weight of the prism of earth ICH, which rests directly on the top of the wall. But, as this displacement is slight, and the influence of this prism but trifling, it is unnecessary to investigate it.

68. That granted, if we first consider the case of sliding upon the base AB of the wall, it is evident, without any calculation, that all revetments of the same height, and which have the same mean thickness, also have, on the specified hypotheses, an equal stability and resistance. Thus, for example, if the point O represented the middle of the outer face of any given revetment, ABCD, of Vauban; and it should be required to transform it into another of the same stability relative to sliding, and with an outer face inclined under any angle whatever; it will merely be necessary to draw, through this middle point, a line with the proper inclination to have the face of the new profile, without at all changing the position of the exterior slope IE of the parapet.

This simple remark suffices to establish the formula,

$$e = 1.625 + \frac{1}{5}(H + h - 2^{m}) - \frac{1}{5}(\tfrac{1}{4}H) =$$

$$(0.2\,a + 0.1)\,H + 1.225^{\text{metres}},$$

which was adopted in number 53. On the same hypotheses, it immediately gives the mean thickness e of a wall with any exterior slope whatever, and of an equal stability with that of the demi-revetments of Vauban. H, h, and a, representing the same quantities as in number 1, and the new berm being regulated in the manner just described.

69. We shall now proceed to the case of rotation, which is the most important in practice (66), and let us undertake to determine, as simply as possible, for a wall ABCD, of the height H, and having the tangent of the inclination of the outer face upon the vertical represented by n, the thickness E, which it must have at the base, in order that its stability may be equivalent to that of a vertical revetment, A'BCD', of the same height, and of an uniform thickness, e. A problem about the same as that which we should have to resolve, if we desired to extend the application of the rules, or tables, presented in the first chapter of this section, to revetments with any slope whatever. If we, moreover, suppose that each wall preserves the same relative position to the common inner face BC, and to the mass of the parapet IEF, we shall have, in order to express the equality of the moments, with references to the edges of the base A and A',

$$E^2 - \tfrac{1}{3}n^2H^2 = e^2, \text{ whence } E = \sqrt{e^2 + \tfrac{1}{3}n^2H^2}.$$

70. To shorten the calculation of this expression, which, by the way, does not in itself offer any difficulty, we shall make use of an analytical principle, which consists in this, that we universally have

$$\sqrt{A^2 + B^2} = \frac{\cos\tfrac{1}{2}(\psi' + \psi)}{\cos^2\tfrac{1}{4}(\psi' - \psi)}A + \frac{\sin\tfrac{1}{2}(\psi' + \psi)}{\cos^2\tfrac{1}{4}(\psi' - \psi)}B,$$

to within a degree of approximation indicated by the value of tang.^{2} $\tfrac{1}{4}(\psi' - \psi)$ for all that extent of the values of the ratio $\frac{A}{B}$ comprised between $\frac{A}{B} = \cot\psi$, and $\frac{A}{B} = \cot\psi'$.

In the case under consideration, where

$$A = e,\ B = \tfrac{1}{2}\sqrt{3}\,nH = 0.5774\,nH,\ \frac{A}{B} = 1.732\frac{e}{nH},$$

the ratio of A to B can become infinite on account of n, $\psi' = 0$. We therefore have the simpler expression,

$$\sqrt{A^2 + B^2} = (1 - \text{tang.}^2\tfrac{1}{4}\psi)\,A + 2\,\text{tang.}\tfrac{1}{4}\psi\,B,$$

to within a degree of approximation indicated by tang.^{2} $\tfrac{1}{4}\psi$.

On the other hand, n, or the inclination of the outer face of the wall from a vertical, is, in new constructions, always less than $\frac{1}{7}$, or even $\frac{1}{8}$; and by the last column to the right in the table of number 34, we perceive that the ratio a of e to H never passes below 0.198, or 0.2; therefore the smallest of the values of $\frac{A}{B}$ will, in all cases occurring in practice, universally exceed 2.5; which number, being considered as the value of $\cot \psi$, the minimum limit of $\frac{A}{B}$, will finally give:

$$\sqrt{A^2+B^2}=0.991\ A+0.1906B,$$

to within the fraction $\text{tang.}^2 \frac{1}{4}\psi=\frac{1}{110}$, throughout all the extent comprised between $\frac{A}{B}=2.5$, and $\frac{A}{B}=$ infinity.

Then we shall also have to within $\frac{1}{110}$,

$$E=0.991\ e+0.11\ n\ H=0.99\ (e+\tfrac{1}{9}\ n\ H) \text{ or } e=1.01\ E-\tfrac{1}{9}\ n\ H,$$

for all cases in which the table of no. 34 is applicable; but the batter of the wall must not exceed one in seven.

71. From the foregoing, nothing is easier than to calculate the thickness E at the base of a revetment ABCD (fig. 4), with its outer face in batter, when we shall have obtained, by means of the table just referred to, or by the practical formula (v) no. 40, if the case permits it, the thickness e of a vertical revetment A'BCD' of the same stability, but with a berm equal to D'I. *Add to e the $\frac{1}{9}$ of n* H, *and subtract from the result the $\frac{1}{110}$ of its value, which, supposing the slope of* AD *to be less than $\frac{1}{7}$, will give to within $\frac{1}{110}$ nearly, the required thickness.*

72. The formula under consideration leads to a method of transformation of profiles analogous to that which has heretofore (68) been presented for the case of sliding; but as it supposes the batter to be less than one in seven, and the ratio $1.732\frac{e}{n\,H}$ of A to B, to be at least equal to 2.5; we have sought to modify it in such a manner as to render it, at the same time, simpler, and more directly applicable in a greater number of cases. If we admit that the thickness of revetments of the same stability, taken at $\frac{1}{9}$ of their height from the base, is within certain limits independent of the slope of the outer face, it is supposing that we have exactly

$$E=e+\tfrac{1}{9}\ n\ H, \text{ or } E=e+0.1906\ \sqrt{\tfrac{1}{3}}\ n\ H,$$

that is to say, $\sqrt{A^2+B^2}=A+0.1906\ B$, for all admissible values of A and B.

But as we have, on the contrary, to within $\frac{1}{110}$ nearly,

$$\sqrt{A^2+B^2}=0.991\ A+0.1906\ B,$$

we see that, if we continue to take the first term of the preceding for-

mula equal to A, it will be necessary to diminish somewhat the coefficient 0.1906 of the second term, which comes to the same thing as reducing slightly the fraction $\frac{1}{5}$, in the formula $E = e + \frac{1}{5} n H$. Now, a numerical discussion shows that, if we replace this fraction by $\frac{1}{10}$, or take

$$\sqrt{A^2 + B^2} = A + \tfrac{9}{10}\, 0.1906\, B = A + 0.172\, B,$$

which approximates to within less than $\frac{1}{64.8}$ of the true value, for all the extent comprised from $A = 2.257\, B$ to $A = \infty$. The error becomes nothing for A infinite, or B=0, which answers to the case of $n = 0$, or a vertical wall. It acquires its absolute and positive maximum value $\frac{1}{65}$ when $A = 5.81\, B$. It returns to zero when $A = 2.82$ B, and then goes on increasing negatively as the ratio of A to B diminishes, which causes it to pass anew by the value $-\frac{1}{65}$, when A is reduced to 2.257 B, or when the ratio of e to H descends to 0.198, at the same time that n becomes $\frac{1}{7}$.

According to the table of number 34, this ratio only falls to 0.198 in the very unusual case where the earth of the embankment unites a very great friction to a very slight density, and is not raised above the top of the wall, which, therefore, does not sustain a superincumbent load. For all ordinary cases, on the contrary, this same ratio will surpass 0.26. We can, consequently, suppose $n = \frac{1}{5}$, without the value of $\frac{A}{B}$, or $1.732 \frac{e}{n H}$, descending sensibly below 2.257.

Then, finally, the formula

$$E = e + \tfrac{1}{10} n H, \text{ or } e = E - \tfrac{1}{10} n H,$$

which is based upon the supposition that the thickness measured at $\frac{1}{10}$ *of the height of the wall is independent of the inclination of the face*, is exact, to within $\frac{1}{65}$ nearly of the thickness at the base, for all cases occurring in practice, wherein we take the coefficient of stability equal, or superior, to 1.912 (18), and do not allow the slope of the outer face to exceed $\frac{1}{5}$.

73. We can satisfy ourselves that this very simple rule for the transformation of profiles is equally correct for the ordinary profile of Vauban, and, *a fortiori*, for that of demi-revetments, as long as the height of the masonry does not exceed 18 or 19 metres, which it never would in practice. This, however, requires that we should increase the new berm in such a manner as to give to the exterior slope of the parapet the same relative position with reference to the inner face of the two walls.

74. Considering, in particular, the demi-revetments of Vauban, for which the thickness at the base

$$E = \overset{\text{metres}}{1.625} + \tfrac{1}{5} (H + h - \overset{\text{m}}{2}) = 0.2\, (H + h) + \overset{\text{m}}{1.225},$$

h being the height of the parapet above the top of the wall, counted from the middle of the superior slope, we shall thus have:

$$e = 0.2\, (H + h) + \overset{\text{m}}{1.225} - \tfrac{1}{10}\, \tfrac{1}{5}\, H = 0.2\, (a + 0.9)\, H + \overset{\text{m}}{1.225},$$

for calculating the thickness, to $\frac{1}{25}$ nearly, of a vertical wall of the same stability. But, observing here that, in practice, as the height H of the masonry rarely surpasses 12 to 13 metres, the ratio of A to B must constantly exceed the number 2.5.—This permits us to take, with still more exactness, or to $\frac{1}{110}$ nearly of E (71),

$$e = 1.01\,E - \tfrac{1}{9}\,\tfrac{1}{3}\,H = 0.202\,(a + 0.89)\,H + 1.237 = 0.18\,H + 0.202\,h + 1.237,$$

and establishes formula (q) of no. 20, as well as the remarks accompanying it; for, in the transformed vertical profile A'BCD' (fig. 4), the width of the berm DI ought, for exactness, (67) to be considered augmented by the quantity DD', equal to $\frac{1}{3}$ of the height H of the wall, less AA', which, being only the $\frac{1}{43}$ of it, can very properly be neglected in the present question.

75. The profile of Vauban is always accompanied with counterforts, which, it cannot be denied, play an important part in the conditions of equilibrium. The question of its transformation into a new wall, also provided with counterforts, and having an equal degree of stability to that of the old wall, has been a subject of frequent discussion among engineers. They have generally assumed, that a like system would turn in an entire block around the outer edge of its base, and that the moment of its resistance to turning was precisely equal to that of its entire weight, independent of that of the earth which lies above it, and, moreover, without taking into consideration the friction of the earth which is filled in between the faces of the counterforts. For the present, neither contesting nor admitting the truth of this hypothesis, we shall content ourselves with remarking:

1°. That, with regard to its stability relative to sliding upon the base of the wall, it will suffice to preserve, for the counterforts of the new wall, dimensions and a position exactly similar to those of the old one, which must be done either with reference to the counterforts themselves and the back of the wall, or with reference to the exterior slope of the parapet, which should always (67) cover the same quantity of the surface of the top of the wall; for neither the pressure of the earth, nor the weight upon the foundation, will have been changed.

2°. That, with regard to its stability relative to rotation, we shall only modify it in an inappreciable manner, if we assign to the counterforts of the new system the same respective dimensions and positions as those of the old one, while, at the same time, giving to the principal mass of the wall, the degree of stability which is called for by the preceding rules; for the edge of the base A' of this mass (fig. 4) will be displaced by a quantity AA', at the very greatest (74) only equal to $\frac{1}{43}$ of its height BC. This only lessens the length of the arm of the lever of the resistance of the counterforts by the same quantity, for which we can always make an allowance by an equal elongation of the counterforts, or by an equal addition to the thickness of the body of the masonry, and without sensibly increasing the expense. While this secures to the system an excess of stability, it permits us

to neglect the influence of a change in the berm, which is only perceptible in the case of high embankments.

Of the Influence of Counterforts, and, more particularly, of the Stability of the Profile of Vauban.

76. As long as the transformation is confined to changing the inclination of the outer faces, without altering the arrangement of the counterforts, we can always secure a degree of stability to the new system, clearly equal, if not superior, to that of the old, without being obliged, as will be shown, to resort to any calculations for that purpose. But, when the question is to replace these counterforts in the new profile by an uniform additional thickness given to the mass of the wall, it is quite a different matter. The difficulty presenting itself in this case is owing to the impossibility of discovering the precise part that the counterforts play in the general system of resistances, arising from the force of cohesion which unites them with the body of the revetment, and from the modification which they introduce into the action of the pressure of the earth.

The researches of M. Audoy shed some light on this point. They lead to this conclusion: That, in order to ensure, to revetments without counterforts, a degree of stability equal to that of the ordinary profile of Vauban, it is necessary to adopt a coefficient of stability equal to 4.70 for the case of sliding, and to 3.80 for that of rotation; so that the substitution of the first of these profiles for the second would be quite disadvantageous in an economical point of view. These coefficients actually being nearly double those which answer (18 and 65) to the main body of the wall; and the thicknesses of revetments with moderate loads increasing nearly proportionally with the values of these same coefficients on the hypothesis of sliding, and with their square roots on that of rotation (38); it therefore follows, that the counterforts must be replaced by an uniform additional thickness which is equal to that of the wall, for the first case, and 0.414 of the same thickness for the second case. Now, this result will appear the more extravagant, because, in the case of very high superincumbent loads of earth, the thicknesses increase in still much more rapid proportions, and because it is the larger number, or the greatest of the increments of thickness, which it will be necessary to adopt, if we wish to give to the profile without counterforts, the same statical properties with the one having counterforts.

77. A like consequence, if admitted in an absolute manner, would lead to proscribing entirely revetments without counterforts, for works of fortification; but it is proper to remark that this consequence depends immediately upon the hypothesis of a tenacity in the masonry sufficient to maintain the counterforts always united to the revetment, in either the case of rotation or sliding; this consequence would also appear incontestible from the calculations of M. Audoy, if we only take into consideration the best of masonry, laid in hydraulic mortar, and thoroughly set; but no such fact can be assumed for the old revetments of Vauban, which are taken for the term of comparison. These were generally constructed of an indifferent masonry, and were

actually far from having acquired, at the time of making the embankments sustained by them, the qualities which the calculations referred to supposed them to possess.

The demolition of ancient revetments by pick, or powder, has, moreover, often offered the example of the counterforts separated from the mass of the masonry, and of their generally remaining standing in the earth after the fall of the main wall: a circumstance which proves, independently of all calculation, not only that the resistance of counterforts to any displacement from their own position, is universally greater than their resistance to separation from the mass of the wall, but also that they offer, against the thrust of the earth which acts directly upon them, such an obstacle as to prevent their being overthrown.

78. Such examples, and, above all, the consideration of the feeble tenacity acquired by the mortar at the epoch when the embankments were usually made, have even made some engineers think that the main part of the wall of the profile of Vauban had, in itself, all the excess of stability which is practically needed. The function of the counterforts being thus reduced to procuring to the revetments of this profile an additional resistance of a purely military character. But this opinion, already of long standing, is in its turn too absolute; and we shall see, in the following paragraph, that it is in direct contradiction with the opinion given by Vauban, in the *Traité de la défense des places.*

79. In the first place, as to the actual part played by the counterforts before the mortar has acquired its maximum tenacity, it is evident, even supposing them absolutely detached from the revetment, they do not the less lend a very efficient support to it:—1°. In diminishing, in a marked manner, the extent of the surface of the back of the revetment subjected to the direct action of the pressure;—2°. In diminishing, likewise, the proper energy of this action, either through the resistance which the sides of the counterforts oppose to the sliding of the earth, or through the conoïdal form which the surface of rupture of the earth is forced to take; or, finally, through the earth forming, in certain cases, a horizontal, or inclined, arch, springing from the faces of the counterforts, which the ingenious experiments of Moreau and Neil show to be possible. Consequently, it is not correct to say that the counterforts exercise no influence, even in the case of dry masonry; and although, in the actual state of the question, it may be nearly impossible to estimate this influence, we must none the less admit its existence.

In the second place, there is nothing authorizing us to think that the profile of Vauban without counterforts could have resisted, in all cases, the action of the pressure of the earth—that is to say, for all kinds of earth and masonry to which it has been applied; we are indeed justified in affirming the contrary, either as to the effects of sliding, or as to the effects of rotation, considered at the epoch when the wall is first finished. It is an assertion we have already advanced, and which it is here proper should be clearly established, without, however, running into any long calculations, or even going beyond

the example offered by the ordinary revetment of ten metres high, with a parapet of only two metres above it.

80. Under the head of sliding, we actually find, in the case for which $p' = p$, and $f = 0.6$, which is not perhaps the most unfavorable hypothesis possible,* that the ratio of the weight of the revetment, and of the earth which covers it, to the effort of the pressure, is solely 1.16; so that the friction of the masonry imperfectly set, seldom exceeding 0.7 of the pressure, the resistance is found reduced, in like cases, to $0.7 \times 1.16 = 0.812$ nearly of the pressure; in this we have entirely excluded any influence of the counterforts. We obtain results much shorter still of a perfect stability, in considering the possibility of rotation; for we then find, for the same kind of earth and masonry, that the moment of the weight of the wall, and of its immediate load of earth, is only the 0.71 of that of the thrust arising from the pressure of the earth. Or, it may be expressed, the reduced thickness, or that at $\frac{1}{10}$ of the height H of this wall (73), which in reality is only equal (20) to 0.342 H, should, from the table of no. 34, be the 0.563 of H, if we wish to insure it a degree of stability which answers to the coefficient $\delta = 1.912$, adopted in the calculation of that table, and only the $\frac{0.563}{\sqrt{1.912}}$, or about the 0.408 of H, if we desire merely a strict equilibrium for which $\delta = 1$; since the thicknesses of vertical revetments sustaining small loads, are sensibly as the square roots of the adopted coefficients of stability (38.)

These last consequences, relative to the possibility of overturning the mean or ordinary scarp of Vauban, would be but very slightly modified even if we take into consideration the increment to the stability which results from the addition of the counterforts; because we have seen (76) that the additional thickness of the masonry which would follow from it, cannot at all exceed the 0.414 of the reduced thickness of the wall, or $0.414 \times 0.342\ H = 0.142\ H$, which gives a total thickness $(0.342 + 0.142)\ H = 0.484\ H$, yet still less by $\frac{1}{6}$ than 0.563 H, the quantity which our table gives; but if it were reduced to its true proportions, it would probably differ very little from 0.408 H, the thickness answering to a strict equilibrium.

81. After that, we cannot certainly maintain that it would be necessary, in the calculation of the table of no. 34, to take equally into consideration the excess of stability due to the counterforts of the ordinary profile of Vauban, and more especially that it would be necessary to replace the coefficient 1.912, which was used in the calculation of that table, by 3.80, the one which M. Audoy decided on. For in the case under discussion in which $p' = p$, $f = 0.6$, there would result a much greater difference still between the stability of this profile even with its counterforts, and that of the one which, it is maintained,

* Some experiments show that certain embankments formed of large round pebbles, and having all the interstices filled with a fine sand, may weigh as much as 2300 kilogs. a cubic metre, with a slope not exceeding 33°, which gives $f = 0.577$; and as the weight of some kind of brick masonry scarcely reaches 1700 kils., we therefore would, in that case, have p' no greater than $0.74\ p$.

is indispensable. As to the rule, which consists in replacing these counterforts merely by an uniform addition to the thickness of the main body of the wall, equivalent in volume to the counterforts, or having the same moment with them—a rule sometimes adopted—we can satisfy ourselves that, for the ordinary scarp, ten metres high, it would, at the most, augment its reduced thickness by the 0.22 of its value in the first case, and by the 0.26 in the second; which would, consequently, be far from offering the necessary chances of stability in the most unfavorable hypothesis to be considered.

82. Moreover, it is not amiss here to recal, that Vauban himself judged his profile to be insufficient under many circumstances. In such cases, he acknowledged the necessity of relieving the walls, until the masonry became set, from the effects of the action of the pressure of the earth. The care with which he then recommended the lessening of this action, either by means of alternate beds of fascines and well rammed layers of earth, or by diminishing the intervals between the counterforts; this care, I remark, is an incontestible proof of his opinion, and one to which it would be useless to add anything. On the other hand, if we wish to consider the most favorable cases to stability with regard to the nature of the earth and masonry, we should then find that the revetment of Vauban possesses by itself such an excess of stability, that the adding of counterforts to it would be running into an extravagance in expense which would not be compensated for by any addition to the purely military qualities of the work.

83. Let us, therefore, once more conclude, that, in order to reconcile economy with solidity in constructions, it is henceforth indispensable to pay strict attention to the actual qualities and character of the earth and masonry, as, indeed, both theory and simple reasoning would indicate. Experiments, which could scarcely have been made in the time of Vauban, can now be tried with great facility. It would be quite inexcusable to confine ourselves to the by no means universally correct hypothesis of mean earth and masonry, even though we should propose, with some engineers, to modify the coefficient of stability according to local circumstances. It would leave too much open to chance, since it is impossible to determine, in any absolute manner, the stability of existing profiles, without taking into consideration the earth and masonry.

On the other hand, if, while continuing to admit the hypothesis of the *mean case*, as it is termed, we attempt to regulate the thicknesses of revetments, after the stability of the profile of Vauban, directly transformed, or replaced by equivalent formulas not less restricted, even though they should be those of the 15th, and following, numbers of this section, it would not be too much, as has been shown, to add, in certain cases, to the principal wall, counterforts such as he prescribed. We would only be justified in replacing them by an uniform additional thickness to the mass of the wall, as long as we should rigidly take into consideration all the essential given quantities of the question, in adopting, for example, the coefficient of stability 1.912, and the method of calculations exhibited in the 36th, and following,

numbers. Because, from the great magnitude of this coefficient, and from the hypotheses settled (2) in favor of the resistance, these methods appear to us to suffice for all the usual cases, and to leave no chance open to uncertainty, even with regard to the sliding of the masonry upon the beds of its courses (66).

(To be continued.)

Mr. Vignoles' Lectures on Civil Engineering, at the London University College.

[Continued from Page 307.]

SECOND COURSE.—LECTURE VI. ON THE GAUGE OF RAILWAYS.

After some preliminary observations, illustrating parts of the last lecture, and particularly in reference to what was stated respecting the Brighton Railway, Mr. Vignoles proceeded to enter on the subject of the breadth, or gauge, of railway, which he explained to denote the distance between the iron bars which form the track, or way. The definition of the gauge of the old tramways, introduced the observation, that, from their form, being, as it were, an artificial rut, they were styled by the French *ornières*, of which the literal translation was "wheel-rut." The present ordinary railway gauge was 4 ft. 8½ in., and some speculations were made as to the choice of such a particular breadth, and quotations were made from Mr. Wood's *Treatise on Railways* to show that it had been owing to an accidental circumstance, viz.,—that the first conclusive experiments on the principle of the present locomotive engines had been made on the Killingworth Colliery railway, which was laid to that gauge. In some of the first of the acts of Parliament for modern railways, it had been made imperative, by a special clause, to adopt this particular gauge, and many companies submitted quietly to the enactment, thereby preventing all chance of improvement in what was assumed to be perfect *ab initio;* but, about six years ago, much discussion having taken place as to the proper gauge, this decree was altered, and there is now no limitation in the width of the gauge, which is left entirely to the discretion of the engineer. Now, the consequence is, that although it would be desirable that there should be a standard gauge fixed, yet, so divided have the public been as to what is the right one, that we have at present no less than seven different gauges used throughout the United Kingdom; some of the Scotch lines, for instance, have a gauge of 4 ft. 6 in., and others of 5 ft. 6 in. The Eastern Counties Company have adopted 5 ft. The gauge of the railways in Russia is 6 ft. On the recommendation of the Irish Railway Commissioners, the Belfast and Armagh Railway Company have made their gauge 6 ft. 2 in. On the Great Western Railway, the gauge is 7 ft. Now, as much as 18 years ago, Mr. Tredgold, a celebrated and scientific engineer, made the following observations:—"The width between the rails being dependent on the height of the centre of gravity of the loaded carriage, and this again varying with the nature of the load and the velocity, it will be obvious we cannot

do better than make the breadth between the rails such that, by disposal of the load, the centre of gravity may be kept within the proper limit in either species of vehicle, whether swift or slow, and it would be desirable that the same breadth, and the same stress on a wheel, should be adopted on all railways. We would propose 4 ft. 6 in. between the rails for heavy goods, and 6 ft. for lighter carriages to go at greater speed." Now, it is remarkable that, during all the discussions that took place with regard to the gauge, this observation was never referred to. When Mr. Brunel broke through that fixed number for the gauge, and adopted another, he gave very strong and sound reasons for so doing; whether he was right in assuming so high a number as seven is questioned by many, but the principle upon which he went was this—"I have (said he) laid out the line as nearly level as possible; the curves that I have adopted are nearly equivalent to straight lines; I keep the centre of gravity low, by placing the body of the carriage within the wheels, and anticipating greater stability and steadiness, I shall be able to go at a much higher speed, and with much more assurance of safety." The Irish Commissioners argue thus—"From the nature of the locomotive engine, its power is so great in proportion to the friction it has to overcome, that it is capable of drawing a load which (even with a greatly increased breadth as compared with common road carriages) extends to a very considerable length, and, in order to reduce this length as much as possible, it is necessary, with the present breadth of way, to make the wheels run within the frame which supports the carriages; the seats of the passengers are, therefore, placed above the periphery of the wheel, which, for the sake of lowering the height of the centre of gravity, is made as small as possible."

One great theoretical objection, therefore, to the narrow gauge, is the increased friction consequent upon the reduction of the diameter of the wheel, since, besides what is due to the load, the friction of a wheel, at the axle, may be said to depend upon the proportion of the diameter of the wheel to the diameter of the axle; but, in attempting to carry out this principle in practice, the axle has sometimes been turned down so small as to produce much greater and more positive inconveniences; and it is very questionable if it be prudent, or desirable, to make the proportion between the wheel and axle greater than 15 to 1, and which proportion can be obtained with 3-feet wheels. Now, with a 4-feet wheel and a 3-inch axle, the proportion being 16 to 1, it may be well doubted if, on this account alone, the large wheels are worth their greatly increased cost. The commissioners, however, urged that the same carriage room may be preserved by extending the breadth of bearing of the rails, so as to allow the wheels to run outside the frame, instead of running within it, in which case we can bring the body of the carriage down to the axle-tree. The gauge may be thus increased from 4 ft. 8½ in. to 6 ft. 2 in. —thus arguing for an increased breadth, that the centre of gravity may be lowered, and the diameter of the wheels thereby reducing the friction, and increasing the power, to overcome the "surface resistance." This is, in other words, getting more leverage; but such an

advantage, however, does not apply so much to railways as to common roads, for, on the railway, there is little, or no, obstacle to be found in the shape of surface resistance, except what are as a few grains of dust compared with the obstacles to be found on the common road, or the deep ruts in a wood, which require very large wheels for the timber wain. "At the same time, (continued the commissioners,) the load itself may be reduced in height, the bottom of the carriage, or truck frame, being, in this case, limited by the axle tree of the larger, instead of the periphery of the smaller, wheel; and, with this reduction of height, the wear and tear will be reduced, and the ease of the motion increased. Moreover, the force to be overcome being less with the same load, we may, by retaining the power of the engine the same, carry a greater load than at present with the same velocity, or, retaining the same load, carry it at a greater velocity by increasing the diameter of the driving-wheels of the engine; or, if it be not desirable to increase the velocity, the speed of the piston might be reduced, which would be a great practical advantage; or, lastly, preserving the same load and velocity, the form and weight of the engine may be made less, and, probably, the one or other of these arrangements would be adopted, according to the nature of the traffic on the railway. Thus, in passenger and mail trains, it might be desirable to increase the velocity, whereas, in the carriage of heavy goods, it would be most economical to increase the load." "But (say the commissioners) there is a point which must be attended to, and that is, that the whole of the advantages apply only to level lines." Now, the Great Western was thus susceptible of having a wider gauge, since the line was made nearly level, for, as the commissioners observe, "in ascending the various gradients and inclined planes, the load has to be raised in opposition to gravity, and the power necessary to effect this is frequently equal to, or exceeds, that which is employed to overcome the friction, and will remain the same to whatever extent the friction is reduced. To avail ourselves fully of the reduced friction, those planes which cannot be worked by assistant power require to be reduced in their slopes, in the same proportion that the wheels are increased, or, otherwise, that assistant power be applied on proportionably less slopes than according to the present practice"—that is to say, that the power of the engine is employed in overcoming the friction of the load, and in raising it up the several ascents, and what is gained by increasing the breadth of the railway and making the wheels run outside the frames, is only applicable to the former, the latter remaining the same as before; "and the advantage of the alteration would be overrated if this circumstance were not taken into consideration." Thus it is that the additional advantage arising from the diminution of friction is so small, when you come to other than nearly horizontal lines, that the advantage is lost. There is yet another reason for increasing the gauge, viz., that we are enabled to construct the machine without being cramped in space for the moving parts, and affording a larger diameter for the boiler; it was this consideration, probably, which first induced practical engineers to pay attention to increasing the gauge above 4 ft. 8½ in. If

we had to begin railways again, we should certainly make the gauge wider than 4 ft. 8½ in. In laying out future lines, particularly where the traffic is not great, the point of consideration will be to obtain the greatest advantage at the least expense, and to determine how much the gauge ought to be increased; and Mr. Vignoles stated, that, after having paid a deal of attention to the subject, he gave it as his opinion that a gauge of six feet would be amply sufficient to satisfy all reasonable conditions. The Irish Railway Commissioners had observed "that, at present, the load is seldom equal to the power of the engine, and, this being the case, but little would be gained by a greater breadth of road," with a view only of reducing the resistance, already much inferior to the power by which it is to be overcome, except by allowing an increased speed on the line generally, and on the level planes in particular. With a full and overflowing traffic, there is no doubt it would be advisable to employ the greatest possible breadth of bearing; but it is useless, or worse than useless, to incur a present expense for a benefit which it is not likely that there will ever be the means of taking advantage of, so that, unless under the cirumstances just mentioned—viz., an incessant traffic—Mr. Vignoles thought that a seven-feet gauge was over the mark. Mr. Vignoles stated that the consideration of curves was connected with that of the gauge, that it was a most important element in the consideration of railways, and would be taken up in another lecture. The rule given for raising the outer rail, on curves, required the gauge to be included as one element in the calculation, as also the height of the centre of gravity above the rails, which was also contingent on the gauge, as before explained.

LECTURE VII.—ON CURVES OF RAILWAYS.

This lecture was devoted to the consideration of curves upon railways, and Mr. Vignoles pointed out the principles on which should be compared the economy and advantages to be obtained by the adoption of curves, with the inconveniences attending on them; the saving of expense in formation, earthwork, bridging, &c., by curving round natural obstacles; the advantages of attaining a more level line, avoiding interferences with valuable property, or approaching towns, mineral or manufacturing establishments, &c., all entering into the former—the practical inconveniences of additional resistance to motion and retardation of velocity, to ensure safety, being the set off; and, among other elements, it was stated that the breadth, or gauge, of the railway affected the calculation. The Professor then showed that along very wide valleys, through champaign countries, and where the grounds undulated, so that the ridges, dividing the watercourses, were successively crossed by the railways at right angles to their general direction, the saving by lateral deviation would seldom be material, and, consequently, that the curves may be laid out so flat as to be practically equivalent to straight lines—the "*accidens de terrain*," to use a French phrase, being, in such districts, to be overcome by cutting and filling, to the extent justified by the importance of the line and traffic, or by the introduction of undulating gradients, some-

what approximating to the natural surface of the country. But, in tracing a line of railway along the sides of hills bounding narrow valleys, particularly where the main valley is broken by lateral rivers, then the economy from curving becomes very great, and the introduction of curves to the greatest possible extent, consistent with safety, is allowable.

Mr. Vignoles then went on to consider the various means employed to obviate the practical inconveniences arising from curves on railways. He began by explaining the peculiar distinction in make between carriage-wheels and axles constructed for running on railways, and those for common roads—in the former the wheel being keyed fast to the axle, and both moving round together—in the latter the axle being fixed to the carriage, the wheels only moving round. Many attempts had been made by engineers to give the railway vehicle the advantage which the road carriage had, of turning with facility and safety round sharp bends, but in vain, as the wheels always got off the rails laterally, at even moderate velocities; it was only on the old tramroads that the wheels were loose on the axles. Railway wheels, being thus fixed to the axles, have the tendency to move on a straight line, so that, on the occurrence of a curve, the effort to continue in motion in the direction of the tangent of that curve, creates a certain degree of resistance, as the wheels are only kept upon the rails by the flanches pressing against the inside edge of the outer rail of the curve. The Professor then entered into a number of technical details, which he illustrated to the class by diagrams, explaining why the flanch of the wheel had now, by common consent, been placed on the inner side of the periphery of the wheel, rather than on the outer side; and also the reason for allowing a certain amount of play, being the difference between the gauge of the rails and the gauge of the wheels, and the manner and cause why the rim of the railway wheel is made somewhat conical—that is, the wheel, instead of being quite cylindrical, is really the frustrum of a cone—stating at the same time, the rule for giving the proper "cone" to the wheel, being dependent on the *minimum* radius of curvature on the line to be traveled over, and the *maximum* velocity. In general, the "cone" was stated to be about one-seventh of the breadth of the rim of the line, giving about one inch for the difference of diameter of the wheels at their inner and outer edge, for, when carriages are passing round a curve, the wheel and axle, being connected, roll together as a rigid body, and require the contrivance of the "play and the cone" to prevent too much lateral friction of the flanch, and to get the wheel round the curve without dragging. Mr. Vignoles then showed that, on the ordinary railway gauge of 4 ft. 8½ in., and in the 3-feet wheels, the above amount of cone and play would be sufficient to meet a curve of only two hundred yards radius, which is greater than any which ought to be laid down on a traveling line for high speeds.

The centrifugal force due to the velocity of the carriage, was next to be considered. As before stated, its tendency in moving round a curve is to keep a tangential course; this force may be accurately computed (being dependent on the velocity of motion, weight of the

carriage, and the radius of curvature) by well-known formula, whence is deduced the fractional part of the weight of the carriage, representing the centrifugal force. The Professor gave the formula, and worked it out on a supposed velocity of something more than seventeen miles per hour, or about 25½ feet per second, on a curve of 200 yards radius, whence the centrifugal force was found to be $\frac{1}{30}$th of the weight of the carriage. Mr. Vignoles quoted the following rules, viz., "multiply the square of the velocity in feet per second by the gauge of the railway, and divide the product by the accelerating force of gravity, multiplied by the radius of curvature in feet," which gave an expression, which, though not the fraction of the weight, was what would do very well for practical and ordinary purposes; it was the height which the outer rail of the way should be elevated, to counteract the centrifugal force, and prevent the wheel flying off at a tangent to the curve. He then stated M. de Pambour's more strictly mathematical, but more complicated, rule for obtaining the same amount of elevation of the outer rail, and showed the table of results calculated by that engineer and by Mr. Wood, of which we only give the extremes, by which it appears that, supposing it safe to encounter so sharp a curve as one of 250 ft. radius, at the rate of 30 miles an hour, the outer rail of the way must be elevated 12 in.; but for a radius of 5000 feet, or nearly a mile, at the rate of 10 miles per hour, the requisite elevation is only one-sixteenth of an inch. Having elevated the outer rail, the axle of the carriage, resting on the two rails, gets such an inclination as will produce on the load a gravitating force inwards equal to the centrifugal force outwards; and there will neither be any tendency in the carriages to upset, nor to press the flanches of the wheels against the rails. The rails once laid, if the carriages run slower than the calculated rate, the centrifugal force is overbalanced by gravitation, and the flanches of the wheel press the inside rails; if quicker, the contrary effect takes place, and the flanches press against the outer rails, so that some medium rate of traveling must be fixed on; and, as the slow trains are in general most heavily laden, any increase of friction has a more powerful effect of retardation than will occur to lighter loads moving at greater speed. Mr. Wood, therefore, advises that the outer rail should not be elevated more than will compensate the centrifugal force produced at the slower rates of motion with heavy trains. Mr. Vignoles then forcibly illustrated the practical effect of neglecting these rules.

He then entered on the subject of laying out curves on the ground, by a succession of set-offs at the end of each length of any given measure—the set-off being calculated from the radius of curvature, considering the given measure (say a chain length) as the side of a circumscribing polygon; and, on the large scale, and practically, a number of these sides of a polygon become the segment of a circle. Mr. Vignoles gave a simple approximate rule for finding the set-off from the radius, or the reverse, by "divide the number 792 (the number of inches in a chain) by the radius in chains—the quotient is the set-off per chain in inches." Thus, the set-off per chain for a curve of a mile radius is 9.9, or, in round numbers, 10 inches. When the curve

is of less than one mile radius, it is advisable to make the set-offs by half-chains. It was observed incidentally by the Professor, from the same rule, the set-off due to the curvature of the earth was, in round numbers, about eight inches per mile, and hence had arisen formerly some curious engineering mistakes, from supposing that a horizontal line was a tangent to the earth's surface; and, in setting out canals, an inclination of eight inches per mile had more than once been given to the water line, while it was imagined it had been laid out for a dead level. In conclusion, Mr. Vignoles mentioned that some further observations on curves would occupy the next lecture.

(To be continued.)

Sectional Sketches of Cast Iron Rails, suitable for Railways of general trade. By CHARLES MOERING, *Captain of Engineers in the Austrian Service.*

The writer—having, in the course of his visit to the United States, had occasion to investigate, with some degree of closeness, the system of railways, and railway communication, at present in action here, upon a development of some three or four thousand miles—was, early in the course of his investigations, struck with the fact that, in the superstructure of their railways, the American engineers *had almost wholly omitted to avail themselves of the abundant supplies of the ores of iron which are to be met with in every quarter of their country, and which, reduced to the simple form of cast iron*, either directly by the smelting furnaces, or recast from the cupola, might—as it appears to the writer—have been used for the rails of most of the railways, with the highest advantage to the prosperity of the internal trade of the country.

And this fact has caused the greater surprise to the writer, from the high and just reputation which the Americans possess abroad, for skilfully adapting their resources to purposes of utility, and for disposing to the best advantage, in their works, the native materials which they have at command.

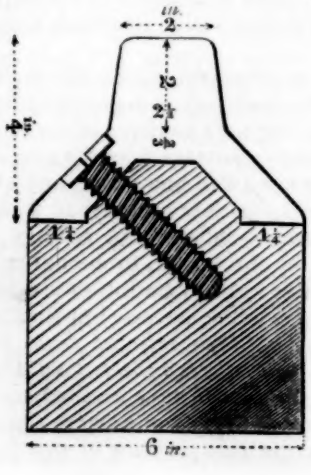
Owing to the circumstance that the government which the writer has the honor to serve, has but recently undertaken a grand system of railway communication, extending to the very confines of the Austrian territories, and considering the facilities possessed by the empire of Austria for the production of cast iron on the one hand, and the limited, as well as imperfect, character of the few established works for producing malleable iron, on the other—information relative to *cast iron rails* was eagerly sought for by the writer, from engineers and others conversant with subjects of this nature; and, after a deliberate examination of the questions which arose, he was impelled to the conclusion, *that cast iron rails had not been rejected from the American railways in consequence of any defects inherent in that material.* This rejection, or omission, appears to have resulted, partly, from the surprising celerity with which these works were simultaneously urged forward—partly, from the inexperience of many of

the engineers necessarily employed, in consequence of the great demand at the time, for men of that profession, having induced a number of unqualified persons to throw themselves into it—partly, from a want of due deliberation consequent upon the rapid progress of the railways, which favored imitation, rather than reflection—partly, from the vigor with which rolled iron rails, *then exempt from duty by the law,* were pushed into use, in every quarter of the country, by interested parties—and, partly, from a long chain of fortuitous circumstances, which conduced to the results we have witnessed, *without deciding the merits of the technical questions involved.*

Convinced that, in America, *cast iron rails* have been without just cause neglected, and that, in Austria, and other countries upon the continent of Europe, which, like America, are deficient in the extensive establishments requisite for rolling rails from malleable iron, that *cast iron rails* might be used with signal advantage, the writer projected a few sections suitable for rails of that material under a heavy trade, and collected some from the civil engineers of this country. Three of these sections are sketched below, which may be found of interest to citizens of Pennsylvania at least, since a select committee of their legislature, with Charles B. Trego, Esq., as chairman—a gentleman whose fitness for such a station is well known and duly appreciated here—in a report adopted by the House of Representatives, and extensively circulated, *have declared unequivocally in favor of the use of Pennsylvania cast iron laid upon a continuous bearing of timber, for the railways of the State, whenever these railways shall require renewal.*

So evident appears to be the policy of the recommendations of the legislative committee above mentioned, that the writer humbly hopes the day is not far distant, when the continental powers of Europe (some of whom are as deeply interested in this matter as Pennsylvania is) will adopt a similar patriotic resolution, and make it applicable to those railways, of which the superstructures are not yet laid down.

Fig. 1.



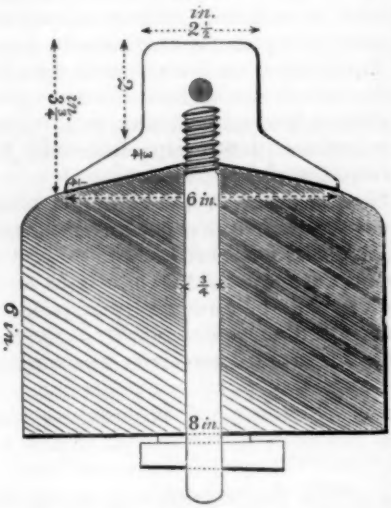
The accompanying section (fig. 1) of a heavy *cast iron rail,* (doubtless susceptible of many improvements,) was devised by the writer in July, 1842, as suitable to the circumstances of the great public railways now under construction by the command of his imperial majesty, the Emperor Ferdinand of Austria. It is designed to be cast in lengths of ten feet, to rest upon a continuous bearing of timber of 6 × 6 inches, to be fastened laterally by good strong screws, (of wrought iron, placed checkerwise,) and weighs about 90 lbs. to the yard lineal. The stringpieces can be easily

shaped by machinery acting upon circular saws, which are to be placed in the proper directions required by the shape of the saddle, or elevation, in the middle of the stringpiece.

Since planning the above rail, some suggestions made to the writer by an able engineer, and further reflection upon the point, have induced the opinion, that the head of the rail ought to project *slightly* upon both sides of the stem, as in Fig. 3; such an outline, while securing abundant strength, would both lighten the rail, and be more favorable to the action of the flanch of the wheel.

The following section (Fig. 2) was sketched for the writer by Mr. Ellwood Morris, of Philadelphia—an American civil engineer, who has devoted a good deal of time to the consideration of *cast iron rails.* This rail also would be the better for a slightly project ing head, as is remarked above.

Fig. 2.



It is designed to be cast in sections of ten feet, to rest upon a continuous bearing of timber, 6 × 8 inches in section; to have imbedded in it centrally, a thick wrought iron wire, or safety rod, to prevent the possibility of accident from breakage, and to be fastened down to the continuous wooden bearing by ¾-inch screw-bolts, three to each rail of ten feet. These screw-bolts will be tapped into the under side of the rails, and keyed up firmly upon a washer underneath the continuous bearing.

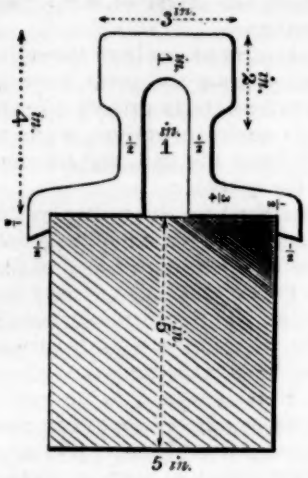
This rail, of which the fastenings will be extremely simple, and secure, will, by its saddle form, be difficult to push out of place laterally, and is designed to weigh about 80 lbs. to the yard lineal.

The following section (Fig. 3) was drawn by the writer in January, 1843, to afford a better protection for the continuous bearing, and with the view of employing it under locomotives with more than *one* driving axle, and *running not more than two tons upon any wheel.*

This rail is also designed to be cast in lengths of ten feet, and to be fastened down to its bearing with three bolts to each rail, upon Mr. Morris' plan; three bosses being cast in the hollow part of the rail to receive the screw-bolts, which will be tapped in as in the other case. Screws may also be used, as with the section Fig. 1.

This rail will weigh about 60 lbs. to the yard lineal, and the continuous bearing of wood will have a section of 5 × 5 inches, with sleepers distant from each other in the clear 1 foot 6 inches, and of a triangular section, with the vertex upwards.

Fig. 3.



More mature reflection upon the proper sectional figure of *cast iron rails*, has induced the writer to think that, to give to a rail of that material a heavy head, projecting half an inch on each side, as in Fig. 3, will be found, in practice, to be the best form for that part; the upright stem must have a good mass of metal, to carry safely the heavy rolling bodies which pass along the surface of the rail; and the base must be broad, to give stability, and distribute the weight over a sufficient surface of timber, which it protects at the same time. Many other outlines of rails may be traced which will fulfil all these conditions, and almost any rail which possesses these requisites will be found useful in practice.

The writer recommends cold blast castings of the toughest kind, run into cast iron moulds, properly heated to prevent any injurious chill; but he believes, however, that it is not necessary to employ a safety rod of wrought iron, as an adjunct to a *cast iron rail*, as he is decidedly of the opinion that *safety* will be abundantly guarantied by the use of a continuous bearing of timber, *which ought to form a leading element in every plan for the use of cast iron rails upon public railways.*

Philadelphia, April 25, 1843.

Report of the Select Committee of the Legislature relative to the Renewal of the State Railways with Pennsylvania Cast Iron Rails.

Mr. Trego, from the Select Committee to whom was referred the memorial of Ellwood Morris and others, citizens of this commonwealth, praying the enactment of a law to provide for the renewal (when required) of the superstructure of the State railways, with rails of Pennsylvania cast iron, made the following report:

That they have given the subject that attentive consideration which they conceive its importance demands, and trust they will be excused for entering into a somewhat extended investigation of the questions raised by the respectable and intelligent gentlemen whose names are appended to the memorial now before the committee.

These memorialists entertain the opinion that, by adopting the use of cast iron rails upon the State railways, when the rails of rolled iron now in use shall require renewing, that the cost of maintaining those roads may be essentially diminished; whilst our own mineral resources will in that case furnish us, in a cheap and simple form, the

material for the repair and renewal of the superstructure of our railways, without involving the humiliating necessity of resorting to a foreign country for railroad iron, as hitherto has been the case.

In the examination of the matter referred to them, the committee have been induced, for the sake of perspicuity, to consider it under the three following heads, into which it may be conveniently divided:—

I.—*Of the rolled iron rails in use.*
II.—*Of the objections to them.*
III.—*Of cast iron rails.*

In the mineral districts of England, railways came into use about the middle of the seventeenth century; they continued to multiply in number and utility throughout that and the succeeding century, and were generally employed upon descending routes for the transportation of minerals to canals, and other navigable waters.

But, as commercial enterprises, they were unable to compete successfully with canals, until the opening of the Liverpool and Manchester railway, in 1829, produced that celebrated trial of speed and strength between steam locomotives, which developed an extraordinary *saving in the time of transit*, as an additional and most important element in their successful progress; and startled the world by demonstrating the fact, that locomotive steam engines could travel with trains upon railways at the prodigious velocity of thirty miles, or more, within the hour—a speed which had until that time been regarded, even by the ablest writers upon the subject of railroads, as absurd in thought, and impossible in practice.

Since then, the great saving of time effected by railways in the transportation of passengers and goods, has been the chief means of extending their use with a rapidity heretofore unprecedented in affairs of this nature, until they have extended throughout the country, and, assuming to themselves the first rank among our internal communications, have bound distant points together with ligatures of iron, along which a doubled speed of transit has virtually annihilated a moiety of the distance between their termini.

It follows, hence, that every proposition which promises a reduction of the expense of maintaining, or an amelioration in the condition of, that mighty agent, the *railroad system*, deserves, from the State authorities, the most deliberate consideration.

With these preliminary remarks, we shall proceed to consider the three separate branches into which we have divided the subject; incidentally observing, however, that we design to confine ourselves to the consideration of that species of rails technically known as *edge rails;* they being of the kind with which the superstructure of the State railways ought to be renewed, whether rolled or cast iron be the material employed in their fabrication.

I.—*Of the rolled iron rails in use.*

From Wood's standard and well known treatise upon railroads, it appears that *rolled iron rails* of the edge pattern, analogous to those

now used upon our railways, were first introduced into England in the year 1820, under the auspices of Mr. Birkinshaw, of the Bedlington Iron Works, who had obtained a patent for an improvement in the form of wrought iron rails, and in the mode of manufacturing them by the aid of the rolling mill.

It would thus seem that these rails were brought before the English public, and pushed into use, by parties interested in their manufacture, under cover of the plausible argument, that, owing to the superior strength and toughness of the material, rails of rolled iron could be made so much lighter than those of cast iron, having equal strength, that an essential economy would result to the proprietors of railways from their use. Accordingly, we find that the Liverpool and Manchester railway, the great prototype of the modern railroad system, was originally laid with rails of 35 lbs. weight to the lineal yard; but, although that railway has only been about fourteen years in use, the whole of the original track has been entirely worn out, and is now replaced with rails of much greater weight, the latest patterns of which, indeed, are understood to weigh nearly 75 lbs. to the running yard.

Now, it would appear manifest, that, whatever economy might have been possessed over a cast iron rail of adequate strength, by the light rolled rails originally laid on the Liverpool and Manchester railway; we may at the least assume that their recent heavy pattern, which time and practice designated, cannot maintain any such pretension.

To resist successfully the action of the ponderous motors now employed in the traction of trains upon railways, a certain degree of mass seems to be indispensable in the rails, whether they be of cast or wrought iron; and it may be useful to inquire what weight would at the present day be given by practical men, to a rolled iron rail for a first-class railway, destined to carry upon its surface locomotive engines of the largest size.

Fortunately, we find this question already answered by one of the highest living authorities, (we mean by Professor Vignoles,) himself a distinguished civil engineer, and the constructor of several important railways, who, in one of his recent lectures before the engineering class of the London University, states, in substance, that, in order to withstand the destructive action of the wheels of the heavy modern locomotives, the top table, or button, of a rolled iron rail, in contact with the passing wheels, ought to have a sectional area of *four square inches*, or a weight of 40 lbs. to the yard lineal, whilst the stem and base of the rail will require to have at least the same weight; so that a suitable rolled iron rail for a first-class modern railway, according to Prof. Vignoles, ought to weigh, at the least, 80 lbs. per lineal yard; and if placed in chairs upon detached supports, the supporting metal thus employed would superadd about 20 lbs. weight of iron per yard; so that, finally, a first-class modern railway, with a wrought iron rail suitably adapted to carry the ponderous locomotives of the present day, if placed upon detached bearings, would, in the opinion of Prof. Vignoles, require the use of 100 lbs. of iron in every lineal yard of a single line of rails.

This opinion of the learned Professor, who is known to be thoroughly conversant with the now existing state of feeling upon that subject in England, clearly evinces that time has already demonstrated the fallacy of the idea formerly entertained, that very light rolled iron rails might safely be used upon railways, and that a great economy would thence ensue.

Since the tariff enacted by the 27th Congress imposes, from and after the 3d of March, 1843, a duty of $25 per ton on all rolled iron imported into the United States, *including railway bars*, which were duty free by former laws, we conceive that the prices of such iron, if of the superior quality which experience has shown that it ought to be, will scarcely hereafter be less than $60 per ton, delivered upon the wharf in Philadelphia, and hence that it would probably be worth nearly $62 per ton, at an average, when delivered upon the Philadelphia and Columbia railway.

Now, when, to the above prices, and the enormous weight of 100 lbs. of iron per lineal yard of rail, prescribed for a first-class railway by Professor Vignoles, we superadd the fact, that the best railways recently laid in England, and in our own country, have continuous bearings of timber for supporting the iron rails *throughout their length*, which must necessarily remove the strongest objection to the use of cast iron rails,—it does indeed appear to be time to recur to first principles, and inquire, first, what is the object of a good railway? To this question Mr. Wood has furnished the answer, by informing us that "the object of all railroads is, to present to the wheels of carriages a smooth, straight, and level surface." We ought therefore to inquire, secondly, how can we most cheaply satisfy these requisites, without compromising either safety or utility?

II.—*Of the objections to the rolled iron rails in use.*

The main objections to the use of imported rolled iron rails in this State, hereafter, will grow out of their expense, *immediate* or *ultimate*, owing to the duties imposed by the present tariff on the one hand, and the deficient durability displayed by these rails on the other; but further reasons, adverse to their use, may be found in considerations of public policy, as we shall hereafter mention.

Rolled iron rails having once been generally introduced upon railways from economical considerations, many other advantageous properties were supposed to be discovered in them, and to be confirmed by experiments, the fallacy of which time has either since shown, or is now rapidly demonstrating.

That rolled iron rails possessed a very superior and extraordinary degree of durability, as compared with those of cast iron, was a leading one of the erroneous notions formerly entertained, which is still prevalent in some quarters, and the entertaining of which may easily be traced to insufficient experience, or to a want of experiments prolonged through a sufficient space of time; and, in fact, it is not surprising that such an error should be incident to the adoption of a new and captivating system of locomotion, of which rolled iron rails were supposed to be an indispensable element.

Mr. Wood, in his well known treatise upon railways, has endeavored to show, by a comparison of the wear and tear of certain cast iron driving-wheels attached to locomotives running upon the Killingworth railway, with similar wheels in action upon the same road, but which were hooped with wrought iron tires, that wrought iron rails would probably surpass those of cast iron in durability, in the ratio of about five to one!

This is truly an extraordinary conclusion, but the universal experience of this country having amply demonstrated, that, while the heavy wrought iron tires of the driving-wheels of locomotives, in full action, require replacing annually at least, cast iron driving-wheels suffer much less wear, and, when applied to similar machines, under the same circumstances of work, will run, essentially unimpaired, for several years together; as has been practically shown on several railways, and indicated in the report made to the Canal Commissioners, in December, 1841, by Thomas Tustin, Esq., Superintendent of Motive Power on the Philadelphia and Columbia railroad. We are, consequently, compelled to reject Mr. Wood's conclusions upon this point, as inaccurate in fact, or certainly as being totally adverse to, and unsustained by, the practice upon railways in this country.

To a similar fate must be consigned the conclusions drawn from certain experiments made upon the Liverpool and Manchester railway, in 1831, for the purpose of showing the extreme durability of wrought iron rails, and from which the strong inference was drawn, that the rails experimented upon would probably last half a century, or more! We have, however, a sufficient refutation of the conclusions drawn from these trials of durability, in the fact that the very rails experimented upon, together with the entire track of which they formed a part, have some time since been worn out, and removed from the road.

Such is the easy refutation of some of the fundamental arguments which have been continually advanced and constantly relied on by the advocates of rolled iron railways, and to which, for that reason alone, we have devoted more space than they would otherwise have deserved.

But a few years since, it was proclaimed by our most prominent engineers, that rolled iron rails, of ordinary patterns, would last from forty to sixty years; and this erroneous sentiment formed a most important element in their estimates of the cost of the maintenance of railways. It now appears, however, that upon this point they were so egregiously mistaken, that it has become extremely doubtful whether any of our railways, which carry much traffic, will witness the coming of their tenth anniversary without a renewal of their rolled iron rails, in whole, or in part.

Upon the subject of the unexpected wear, or deficient durability, of rolled iron rails, we will now observe, that it appears from the report of a late meeting of the British Association, published in the London Athenæum, that Mr. Braithwaite, a civil engineer, had called the attention of the mechanical section to the fact, that the rails of the North Union railway, which was opened for travel as recently as the

year 1838, *had already required turning.* From this it appears that but four years' use upon that railway, by an extensive traffic, drawn by the heavy modern locomotives, had sufficed to destroy the top tables of the rails, and render them to a great extent unserviceable,—though, from another source, we learn that they weighed 60 lbs. to the yard, or actually more than the heaviest of the rolled iron rails in use upon the railways of this State.

It should be further remarked, that the expedient of reversing damaged rails can be resorted to *but once*, and that, therefore, it seems certain that those of the North Union railway, notwithstanding their weight, will be entirely destroyed within less than eight years from the date of their first use.

Upon the railway between Baltimore and Frederick, in Maryland, where a new track was laid a few years since, with rails of 50 lbs. weight to the yard, a very extensive peeling off of their top surfaces may be noticed, and many have already been *turned.*

The same evil exists, to a considerable extent, upon our Philadelphia and Columbia railway, as was officially announced to the legislature by W. K. Huffnagle, Esq., the engineer in charge of that road, through the report of the Canal Commissioners for 1841.

In point of fact, both upon the State railways and those owned by companies in Pennsylvania, wherever they have been much used, ample evidence may be seen that the rolled iron rails employed, are rapidly undergoing a process of disintegration, and will, ere long, become so much crushed and laminated, that traveling upon their surfaces will cease to be either safe or practicable.

It would be an easy matter to extend this branch of the subject by a particular reference to several railways; but this course seems to be scarcely necessary, and might be deemed invidious.

It may be alleged by some, that the rapid wear and tear above alluded to, might be obviated by manufacturing the rolled iron rails, hereafter, in a superior manner to that which has been heretofore employed; but it is easy to conceive that increased care in manipulating the iron, or a repetition of the purifying processes, would necessarily enhance the cost to an extent which, when added to the duty, might very probably augment the price per ton to an amount considerably beyond that which we have before referred to, as being the probable limit hereafter.

From the nature of the manufacture, rolled iron rails are made up of laminæ, either disposed vertically or horizontally; the latter is probably the best mode, and would make the most durable rails, but it merely alters the process of disintegration, and does not annul it; for, if rolled with the laminæ vertical, the rails split and crush; and if horizontal, they exfoliate and scale off under the passing wheels, though the latter would seem to be a less rapid process of destruction than the former.

Even if rolled iron rails should be hereafter manufactured in a superior manner, they would still be liable to a new objection, seriously affecting their strength; namely, the crystalization of the metal by the concussions produced from the action of the wheels. The attention

of practical men has recently been strongly attracted to the consideration of this strange and injurious property, in consequence of the occurrence of an unusually fatal accident upon the Versailles railway, in France, where, owing to a sudden fracture, when in motion, of a locomotive axle, originally made of the very best fibrous wrought iron, but which had crystalized by use and become brittle, more than sixty lives were instantly lost.

A close examination of analogous phenomena seems now to have settled the question, that, no matter how tough and fibrous a piece of iron may originally be, yet, if it is subjected to a long series of small shocks, the particles of the metal inevitably re-arrange themselves into a highly crystaline form, and fracture quickly follows.

All railway iron, in use, is subject to incessant percussions, and, consequently, must hereafter be regarded as being peculiarly liable to the foregoing objection.

III.—*Of cast iron rails.*

It has been so frequently shown in the arts, by the successful use of *cast iron* in the construction of bridges, roofs, girders, the principal parts of steam engines, and almost every species of machinery, that this metal is fully competent to maintain the most prodigious strains which pressure can impose, that it would be absurd in any one to argue the reverse; and hence those who object to the use of cast iron rails for railways, have usually confined themselves to the allegation, that, as cast iron is not well adapted to resist shocks, that, therefore, it cannot be made safely to withstand the percussion of railway wheels. This objection, however, is only true to a certain extent, and the error is, that it has been too broadly enunciated, without taking the pains to learn the character and extent of the shocks to be encountered in practice.

The idea is generally prevalent that it is impossible to measure the strains imposed upon the rails of railways by the impact of the driving-wheels of locomotive engines, when moving at great velocities; but such is not the fact,—for we find it stated in an article "*On Cast Iron Rails for Railways*," written by Ellwood Morris, civil engineer, for the Journal of the Franklin Institute, November, 1842, that the distinguished Professor Barlow has shown, by actual experiments upon the Liverpool railway, conducted with the most delicate instruments, and with the greatest care, "that the vertical stress imposed upon a railway by the transit of locomotive engines, at velocities varying from twenty-two to thirty-two miles an hour, is but little, if any, in excess of that produced by a quiescent load of the same weight!"

Professor Barlow further remarks, that as, from unavoidable imperfections in the road, a strain is sometimes thrown upon a single wheel, equal to about double the quiescent load, as was indicated on several occasions by the deflectometer, during the progress of the experiments, it must therefore be regarded as "an experimental fact, that, with engines of twelve tons weight, (and three tons on a wheel) running at velocities not exceeding thirty-two or thirty-five miles per

hour, it is not necessary, even as railways have been hitherto constructed, to provide for a strain of more than seven tons, which is allowing a surplus strength of sixteen per cent. beyond the double of the mean strain."

It would therefore seem to be an inevitable conclusion, that, even with detached bearings, if we were to give to a cast iron rail the strength necessary to sustain, with safety, *a quiescent load of treble the weight designed to travel it upon the heaviest loaded wheel*, as has been already prescribed by Mr. Nicholas Wood, we should impart to it a power fully adequate to its purpose.

But we would go still further, and propose that, "to make assurance doubly sure," cast iron rails ought always to be laid upon a continuous bearing of timber, and perhaps also to have imbedded in them centrally, a thick wrought iron wire, as has been suggested by Mr. E. Morris, in the article above referred to, with the view of preventing the possibility of separation between the parts of a cast iron rail thus laid upon a continuous bearing of wood, even if it were fractured, either by design or accident, into many pieces.

That cast iron rails may be safely used upon railways, if of suitable metal, adequate dimensions, and laid in a proper manner, would seem to be rendered certain from the successful experience already had, under rather unfavorable circumstances, with the cast iron rails of the turn-outs upon the State railways, which are mentioned in the memorial committed to us, and which have for that reason been particularly considered.

We find that these rails have a transverse section resembling an inverted U, cast solid, the base being about six inches broad, and seven-eighths of an inch in average thickness, while the body of the rail upon which the wheels run is about two and a half inches wide and two inches high, clear of the flanched base; the sectional area of the profile being about 10½ square inches, and the consequent weight of a rail about 100 lbs. to the yard lineal.

These rails are fourteen feet in length, and are laid, and move, upon *detached bearings* of iron, projecting from a bed-plate, at an average distance of twenty inches asunder; many of them were laid down with the rest of the road, and have been in use since 1835, wearing in a perfectly satisfactory manner, and being now, as we are informed, in a better condition than the contiguous rolled iron rails, though this might perhaps have been expected, from the great weight of the movable rails of the sidelings.

If these rails, forming, as they do, sections of single track fourteen feet in length, at many points of the road, and being, from their position, liable to unusual wear, had been inserted merely as an experiment to test the strength and durability of cast iron rails, that experiment would undoubtedly have been regarded by all as signally successful; and this committee cannot perceive the reason why these rails, succeeding as they do in detached sections of fourteen feet, at many parts of the road, would not answer equally well if brought together in a single line, or if laid continuously for miles to form the track of a great public railway.

Though the rails above referred to weigh about 100 lbs. per lineal yard, still their success upon *detached bearings* has been so satisfactory, that we must regard it as a fair inference that, with the precautions indicated by us, rails of the solid U pattern, of 80 lbs. weight to the yard, if laid upon a continuous bearing of timber, would equal rolled iron rails of the weight of 60 lbs. per yard, and would be amply sufficient for any of the State railways.

In the present state of the experience upon railways, it is probable that, if the State roads were now to be re-laid, a rolled iron rail, if used, would be laid upon a continuous bearing of wood, and have a weight of at least 60 lbs. to the lineal yard, though even that is considerably lighter than Professor Vignoles has declared to be suitable to the modern practice: and since a cast iron rail of one-third greater weight would equal, or surpass, it in strength, we may with propriety assume that a cast iron rail, of 80 lbs. weight per yard, could be advantageously substituted for a rolled iron one of 60 lbs. to the yard.

As cast iron rails would not cost more than $40 per ton, delivered, let us now compare the probable relative cost, per mile, of a single track railway, laid with each kind of rails, as follows:

94 tons of rolled iron rails, (60 lbs. per yard,) at $62,	$5,828
125 " of cast iron rails, (80 lbs. per yard,) at $40,	5,000
Saving per mile of single track railway,	$ 828

So that, if we may in this way save even eight hundred dollars per mile of single track, it would, in relaying two tracks of the length of the Philadelphia and Columbia railway, effect an aggregate saving of nearly $130,000.

With regard to the probable expense of maintenance in a cast iron railway, Mr. Zimpel, a German engineer, in a recent work on railways, states, on the authority of an English civil engineer of distinction and experience, that cast iron railroads may be kept up with but a moiety of the outlay required to maintain those of rolled iron.

It has been alleged by some that, upon a cast iron railway, the adhesion of locomotives would be so deficient as to form a strong practical objection; but from a consideration of the loads drawn upon the Philadelphia and Columbia railway, by a locomotive engine with cast iron driving wheels, noticed in an official report by Mr. Tustin, the Superintendent of Motive Power, as being equal to those drawn by other engines; from the experiments upon the sliding friction of iron which have been made by philosophers in Europe; and from the practice in those railways here, where cast iron driving-wheels have long been successfully used—we cannot regard this objection as having much weight, because the loss of adhesion, if any, must necessarily be slight.

It would be easy to reinforce, by further arguments and other illustrations, the favorable position which we have taken towards cast iron rails, but the extent to which this report has already pro-

longed itself, induces us now to conclude with a few more brief observations.

Concluding Remarks.

In the earlier stages of the organization of the modern railroad system, it was doubtless politic in the general government to encourage the extension of this novel and rapid mode of intercommunication, by exempting railroad iron from duty; but, in the progress of events, the time has now arrived when Congress have wisely taken away that boon, never again, we trust, to restore it; for the day has come when the admission of iron, duty free, no longer benefiting the country to a commensurate extent, would simply be the means of enriching foreign iron masters at the expense of our own native industry; and with the full knowledge of the wants of railways, derived from actual experience, we may now recur with confidence to our own mineral treasures as a source for the supply of our own railways with our own iron.

The statistics derived from the census of 1840 announce the striking fact, that of the 286,903 tons of cast iron now annually manufactured in the United States, 98,395 tons are produced by the 213 furnaces of Pennsylvania alone; an amount of cast iron which, whilst it is more than one-third of the aggregate product of this metal annually in all the states and territories, is nearly treble the quantity manufactured by any other single state in the Union.

Here, then, is an interest of vast importance, which may be nurtured and promoted by the adoption of cast iron rails for railways; and surely the highest considerations of public policy should prompt the authorities of such a State as this, to foster and encourage a leading branch of manufacture from our own native material, by requiring the use of Pennsylvania cast iron upon the State railways, even if the cost of rails of that material should exceed, and not fall short of, that of others.

Finally, the committee would observe, that though they fully concur with the views expressed in the memorial, as to the expediency of the proposed measure, yet, considering the near approach of the period fixed for the adjournment of this legislature, and the amount of public business which yet remains to be disposed of, they have not thought it advisable to report a bill. Neither do they conceive any immediate legislation on the subject to be actually necessary, inasmuch as the Canal Commissioners have full power to act in the case, and to direct in what manner the State railways shall be hereafter renewed.

To that Board, then, they respectfully and earnestly recommend a thorough and impartial investigation of the subject, and an attentive consideration of the arguments and facts stated in this report, believing that they will see the expediency of renewing, with rails of Pennsylvania cast iron, at least so much of the State railways as may serve fairly to test, by actual use, the comparative merits of cast and of rolled rails, when subject to equal circumstances of trial. They therefore offer the following resolution:

Resolved, That the committee be discharged from the further consideration of the subject.

Report by Major General Pasley, on the Breaking of Axles, and other Causes of Accidents upon Railways generally.

RAILWAY DEPARTMENT, BOARD OF TRADE,
Whitehall, 23d December, 1842.

My Lord—After the unfortunate accident which took place on the 8th inst., on the London and Birmingham Railway, I went to the Vauxhall terminus of the South Western Railway, to inquire into an accident, unattended with personal injury, which took place on that railway on the 10th inst., from the breaking of a crank-axle of one of their six-wheeled locomotive engines. On inspecting the fractured iron, it seemed to be of good quality, but had been made in an improper manner, by welding flat parallel plates together, contrary to the usual and more perfect custom. At that station I had an opportunity of comparing six-wheeled and four-wheeled locomotive engines, and I have made the same comparison since on the Croydon Railway; and I now beg leave to submit to your lordship the result of my observations and reflections upon railway accidents, whether depending on the construction of the locomotive engines, or other causes.

1. ***The usual construction of locomotive engines compared.***

Four-wheeled engines, with inside bearings, are exclusively used on the London and Birmingham Railway, in compliance with the opinion of Mr. Bury, the superintendent of the locomotive department of the company; and, as the axles of those engines had often been broken without any disastrous result, and that railway (which is extremely well managed) had been as free from accidents, if not more so, than any other; I did not feel myself warranted in making any official remarks in depreciation of four-wheeled engines, until the recent accident of the 8th instant. In that accident, the fore axle of one of their four-wheeled engines broke off transversely close to the inside of the nave of the wheel, so that the wheel was thus, as it were, entirely cut off, and got away from the engine, which, therefore, by having three wheels only, lost its former direct motion, and moved round with a circular motion, which threw it off the rails and carried it down an embankment, where it rolled over and was found with its head reversed, that is, pointing in a contrary way to its original course, so that it must have described a semicircle before it stopped. Further particulars relating to this accident were reported in my letter to your lordship of the 13th inst.

In conversing upon the subject with Mr. Bury, and with Mr. Creed, the Secretary of the London and Birmingham Company, these gentlemen both declared their opinion that the same would have happened to a six-wheeled engine, under the like circumstances; but there were none but four-wheeled engines on that railway, so that I

could not make a personal comparison of the two sorts at the period of the conversation alluded to. Having since done so with great attention, as before mentioned, and having allowed myself ample time to consider the question, I think it my duty, with due respect for those two gentlemen, to record my dissent from their opinion.

In the case of any of the axles of a four-wheeled engine, of the London and Birmingham Railway pattern, snapping off short close to the inside nave, the wheel must necessarily get away altogether, there being no outside framing to confine it.

In the case of the fore axle of a six-wheeled engine with outside bearings breaking, on the contrary, as the journals are outside, the fracture must necessarily be between the bearings; in which case the experience, not only of six-wheeled, but even of four-wheeled, engines proves that no serious danger is likely to result. It has been urged, and I believe justly, that, when an axle breaks between the bearings, the wheels are more deranged, and more liable to leave the rails, by the outside, than by the inside, mode of fixing; but the fore wheels of a six-wheeled engine with outside bearings cannot get away altogether under any circumstances, unless an axle were to snap short off in two places at once, close to, and on, each side of the nave of one of these wheels, which is impossible. Setting aside, therefore, the supposition of a double fracture, if the fore axle of a six-wheeled engine breaks, no matter where, the wheel is confined closely between the strong frame of its outside bearings and the smoke box, which is also sufficiently substantial, so that it cannot possibly disengage itself entirely. Hence the train may be retarded and stopped, but the engine can neither break down, nor be thrown violently off the rails; in short, no accident injurious to the safety of passengers is likely to occur. If the crank, or centre, axle of a six-wheeled engine should be fractured, the fore and hind wheels alone are capable of supporting the whole weight of the engine, so that it cannot break down. If the hind axle of a six-wheeled engine should break, and even supposing an impossibility, namely, that both the hind wheels should come off, no result injurious to the public safety can be apprehended, because the centre of gravity of locomotive engines of this description is a little in front of the centre axle, and therefore the engine will still remain in the satisfactory state of stable equilibrium.

Having thus stated my opinion, and the grounds on which I have formed it, as to the superior safety of six-wheeled engines, I beg leave to report on the nature of accidents likely to be attended with danger to railway passengers.

2. *Collisions of trains, or locomotive engines.*

Collisions of trains meeting each other, which are the most dangerous, cannot take place on railways having double lines of rails, if the switches are self-acting, and such that no train going at full speed can quit its own line, on coming to the points where the crossings are laid out, by any carelessness of the engine-driver.

Collisions from one train running into another from behind, cannot possibly be prevented by any mechanical means. If the regulations

of the railway company are good, and their enginemen are skilful and possessed of judgment and presence of mind, such collisions cannot happen, except in the case of intoxication. Sobriety is therefore indispensable in this class of men, for, without it, all other qualifications go for nothing.

3. *The breaking down of locomotive engines, or railway carriages.*

If a locomotive engine, or its tender, breaks down at the head of a train, the carriage immediately in the rear of the tender suffers the most, and the second and third carriages also may be more or less damaged. Generally, in cases of this kind, the carriage next to the tender is dashed to pieces, or nearly so; and the passengers, when any have been in it, have been killed, or severely injured, whilst those in the next carriages have either escaped, or been less injured. Hence the expedient of placing an empty carriage of some sort next to the tender has been generally, and I think justly, deemed conducive to safety. A truck, loaded with hard materials, such as iron, stone, &c., placed next to the tender, might, on the contrary, do infinitely more harm than good. It has been suggested that a peculiar carriage, with more powerful buffer-springs, or, as it were, all buffer-springs, would be desirable, instead of a common carriage, and I believe so; but it is still more important to adopt such measures as shall do away with the risk of locomotive engines breaking down, or being suddenly stopped by some obstacle capable of producing that effect.

4. *Slips in cuttings and embankments.*

These have invariably arisen from the slopes having originally been made too steep, perhaps for want of experience on the part of railway engineers, who had, as it were, to create a new art, for they have never in the first instance allowed a slope of more than two to one, even in the worst descriptions of soil, which I should myself, when new to the subject, have considered ample; but from subsequent experience and observation, I am of opinion that, in all deep cuttings in plastic clay, slopes of four to one on one side, and of three to one on the other, ought to have been allowed; for, though both sides are liable to slip in such soil, yet one side is usually more exposed to this injurious action than the other, owing to the inclination of the strata. But it may be doubted whether, in all cases, inexperience on the part of railway engineers could have been the cause of their adopting insufficient slopes; because, in Sir Henry Parnell's book on road-making, published so far back as 1833, it is laid down as one of the rules, &c., professedly derived from the practice and experience of the late eminent civil engineer, Mr. Telford, that in the London and plastic clay formations, it will not be safe to make the slopes of embankments, or cuttings, that exceed four feet in height, with a steeper slope than three to one. As most of the great railways have been executed since that period, in some of which the depths of cuttings are from 15 or 20, and even 70, feet, and the heights of embankments also very considerable, a query occurs, whether inexperience could have been altogether the cause of neglecting so salutary a maxim. May not, in

some cases, the apprehension of deterring the public from subscribing to a new railway, have caused a steep slope to have been adopted in preference to a flatter one, because the latter would have made the estimated expense of the removal of earth much greater? However that may be, experience has shown that, after embankments have once come to their bearings, slips are not to be so much apprehended as in deep cuttings. In the latter, laid out, as before observed, with slopes not exceeding two to one, experience has proved that slips must be expected for years after the opening of the railway. The repeated slips on both sides of the Croydon Railway, for example, took place nearly two years, and that on the west side of the Bug-book cutting, on the London and Birmingham Railway, more than four years, after these lines had been opened; and in both cases, notwithstanding that both lines of rails were completely buried by the immense masses of earth thrown down, yet no danger to passengers resulted, owing to these cuttings having fortunately been carefully watched. This precaution, therefore, of continued and careful watching, is absolutely necessary, in respect to embankments and cuttings in plastic clay, especially the latter; for, if not discovered in time, a very insignificant slip of earth from one of the sides of a cutting, though capable of being removed in a few hours, might, by suddenly stopping an engine at full speed, endanger the lives of the passengers in the carriages nearest to the tender—an instance of which actually occurred on a different railway about a year ago. But if the state of cuttings and embankments be always carefully attended to, no serious accident is to be apprehended from any slip, however considerable. I shall only further observe, that it is well known that not merely the nature of the slopes, but also the proper drainage of cuttings and embankments, are essential considerations, both in preventing slips in the first instance, as well as the repetition of them in unfavorable soil.

5. *Of the axles and wheels of railway carriages.*

The breaking of an axle of a locomotive engine is, I believe, more common than the public generally suppose, and, though seldom fatal, it sometimes may be so, as in the case of the accident of the 8th inst. on the London and Birmingham Railway; and though the fracture of an axle of one of their four-wheeled locomotive engines, in that peculiarly dangerous manner, has only occurred once in six years, yet, admitting the possibility of a precisely similar fracture of any of their axles occurring again, the same result must inevitably follow; but I see no mode of guarding against it in four-wheeled locomotive engines with inside bearings, except by adding a light outside framing on each side, which need not have regular brasses like those attached to the present inside frame, nor need they act upon the axle when the engine is in a perfect state; but, though not in contact with it, they should be so near as to come into play whenever the axle breaks. By this arrangement, the wheels would not be able to get away under any circumstances. I have seen a similar arrangement adopted on the South Western Railway, in respect to the fore axle of one

of the six-wheeled engines with outside bearings and journals, by John V. Gooch, their superintendent of the locomotive department, who has attached horns, as they are technically termed, inside of the wheels, ready to act should the axle break, but not otherwise; a precaution, however, which, though it may be prudent, is not absolutely necessary in the six-wheeled engine. I have also seen the crank axle, or centre axle, of a six-wheeled engine made with double bearings, springs, and brasses, both in one of Mr. Robert Stephenson's old engines, and in one at the South Western Railway, and also in Sir John and Mr. George Rennie's new engines; but, on the whole, after having carefully considered this subject, I came to a conclusion that six-wheeled engines, with outside bearings for the fore and hind axles, and with inside bearings for the crank axle, would be a convenient and sufficiently safe arrangement, especially if horns of the description before mentioned be used as a sort of guard for the fore axles in the event of a fracture. After having formed this opinion, I was informed that Mr. Gray, of the Hull and Selby Railway, whose engines I have not yet seen, had previously adopted and actually carried it into effect. Under these impressions, I do not think that Mr. Robert Stephenson has made a change for the better in his new patent six-wheeled engines, in which he has adopted inside bearings for all his axles. I also consider the want of flanches on his centre wheels (which is a peculiarity in that gentleman's engines) to be rather a defect than otherwise.

6. *Hollow axles, &c.*

The axles of the engines and carriages of railways are usually tested in a manner more or less severe, and those which fail are rejected; but it is believed that, though good at first, they become deteriorated in time, not merely by wear, but that the metal gradually loses the proper fibrous texture of malleable iron, and becomes crystalized and brittle like cast iron. Besides magnetic influence between the brass journals and the iron, which has by some been considered the chief cause of this deterioration, the axles of railway engines, &c., are liable to two sorts of injurious action; first, the jolting or jumping at high speed, owing to little inequalities in the rails, which is equivalent to cold hammering; secondly, to torsion in going round curves, or otherwise. In traveling upon six-wheeled and on four-wheeled engines, I have found the former generally smoother, so that the action of percussion appears to me to act with greater violence on the axles of the latter. Mr. John Oliver York has recently taken out a patent for hollow axles, in which only two semicircular plates of malleable iron are welded together, instead of a great number of bars being united by the same process, to form a solid axle, according to the usual custom. His opinion is, that it is the original welding chiefly that injures the iron; but whether his theory be correct or not, I have been informed that, on comparing his hollow axles, of 4 inches exterior diameter, with solid axles of 3½ inches diameter, by subjecting both to the blows of a 38 lb. hammer, the former were found to possess much greater resistance, though containing rather less iron. If the

hollow axle should show the same superiority over the solid one, in its resistance to torsion, as it has evinced in respect to percussion, (an experiment which is about to be tried at the Camden Town station of the London and Birmingham Railway, at the suggestion of Mr. Bury, who considers the former to be the most injurious action of the two,) I think it probable that hollow axles will generally be adopted by all railway companies, as soon as their present solid axles shall require to be replaced by new, which must of course be done sooner or later. In respect to the working, or cranked, axles of locomotive engines, the hollow pattern is entirely inapplicable, but it may be used for all the other axles of locomotives, and all the axles of tenders, carriages, wagons, and trucks upon railways, without exception; and, upon the whole, I consider it very promising.

7. *Of railway signals.*

These are nearly assimilated on all the railways that I have inspected; the red color, or light, being the signal of danger, requiring a train to stop, and the green being the signal of caution; but in some railways, the red light in the rear of the hind carriage of a train is placed so low that it may be obscured by some intervening object, and consequently not seen by the engineman of another train, or of a special engine, following, which was the cause of a collision on the Croydon railway. It is therefore desirable that all railway companies should adopt the system, of which the principal ones have already shown an example, by placing on the hind carriage of every train two red lights, one on each side, so high that they cannot be obscured; and, on the lines alluded to, the low central red light has also been retained, though less necessary. In fogs, a red light is sent out from a station to a sufficient distance to warn the coming trains to slacken their speed; and in the case of a train breaking down, or being obliged to stop, at some intermediate point between two stations, a man is sent back with a red light as a warning to the next coming train to stop, in order to prevent collision. On the London and Birmingham railway, a very ingenious plan has lately been adopted in case of fog, or danger; that is, to send a man to place a small tin box, containing a charge of gunpowder, with a little fulminating powder, on the line of rails by which the next train is advancing; as soon as the wheel of the locomotive engine of which passes over this box, it fires the charge, with an explosion sufficiently loud to be heard in the most stormy night, but not powerful enough to injure the rails, or engine. Under the circumstances supposed, this arrangement, as a night signal of danger or caution, is evidently preferable to a red light, because no neglect or inattention on the part of the engineman, stoker, &c., can render it possible for them to pass on without being aware of the explosion, which can never fail to take place; but the combination of both might, perhaps, be still better than either of these expedients singly.

8. *Of untried inventions for preventing accidents on railways.*

A great number of plans of alleged improvements in railway engines, carriages, and signals, and in the rails, switches, &c. &c., with

a view to safety, as also for preventing slips in cuttings and embankments, have been brought under my notice in the course of the present year, (many of them extremely ingenious, but some very complicated,) by their respective inventors, to whom I have invariably returned the same answer, namely, that I shall be happy to see their inventions, for the sake of personal information, but that I decline recommending them, or even giving an opinion upon them, as the adoption of such plans must depend entirely upon the directors of railway companies, upon whom the Board of Trade will not urge any experimental arrangements involving expense.—I have, &c.,

C. W. Pasley, Major General,
and Inspector General of Railways.

The Right Hon. the Earl of Ripon, &c. &c. Railway Mag.

On the Mode of Calculating the Strength of Cylindrical Steam Boilers. By B. H. Latrobe, C. E.

To the Committee on Publications:

Gentlemen—In the January number of the Journal is an article by Thos. W. Bakewell, Esq., in which the memoir of the explosion of the steamboat Medora, prepared by me at your request, and published in November last, is referred to as "another instance of respectable authority to a prevailing and dangerous error in estimating the capabilities of cylindrical boilers to sustain a given pressure of steam." I have read Mr. Bakewell's remarks with respectful attention, and a desire to discover the error (if any) which he alleges to exist in the mode of computation which I had adopted, and for the truth of which, as he acknowledges it to be the "received rule," I can be under no special responsibility. I am unable, however, to perceive wherein lies the supposed mistake in the formula which I used, and which is expressed by the equation $x = \frac{2Pt}{D}$; where P is the cohesive force of the metal of the boiler per square inch—t, the thickness of the shell in inches—D, the diameter of the boiler, also in inches—and x the extreme pressure, per square inch, that it will bear. I now offer the following remarks as expressing my confirmed conviction, with the reasons therefor, that the rule is right, Mr. Bakewell's belief to the contrary notwithstanding.

Let us look into the conditions of the question. In what manner does an elastic fluid, pressing equally in every direction within a hollow cylinder, act to produce its rupture? Plainly by tending, in the first instance, to drive before it, in a radial direction, perpendicular to the surface of the cylinder at any point which may be selected, each particle, or rather line of particles, forming its thickness at that point. This is the primary action of the pressure upon each individual atom of the shell. But it is manifest that the forces tending to separate any two of those atoms contiguous to each other, are not identically those just named, but a *resultant* from them; for they themselves operate

on those two adjacent particles, in lines making so small an angle with each other as to be, in effect, parallel; so that the particles would be driven off side by side, instead of pursuing opposite directions, as they in fact do in the case of fracture. Moreover, how small soever this angle may be supposed, inclosing, as it is assumed to do, only the breadth of an atom at the periphery of the cylinder, it will, in embracing the same atom, increase its opening as the radii diminish; so that, if the particle were torn asunder directly by these very radial forces, their action would obviously become more powerful as the diameter of the cylinder lessened, which is in opposition to the admitted fact that, the less the diameter, the *greater* the strength, of the vessel. What, then, is the resultant of these radial forces which has just been referred to, and of which we are in search? In seeking an answer to this question, we must bear in mind that, in tearing a body of any kind asunder, two forces, in some degree superior to its cohesion, must be exercised upon it in *opposite* directions; otherwise (unless the two were in exact equilibrium) it would move *bodily* in the direction of the greater force. The two halves of the cylinder in question are thus urged with equal energy, by the outward pressure of the fluid, towards opposite quarters, and we have only to consider the strains to which the shell is subjected at the two ends of any one of its diameters. Now, these strains are equal to the sum of those of the components of all the radial forces on the semicircumference which act tangentially to the cylinder at the points strained, and they are to be estimated by multiplying the unit of pressure by the *diameter*; for the sum of the radial pressures being represented by the semicircumference to which they are perpendicular, the diameter will represent the sum of those perpendicular to itself, and the radius that of those parallel thereto, according to the parallelogram of forces—each radial line of pressure being resolved into one parallel to the diameter, and unproductive of effect upon the parts strained, because its direction is at right angles to that in which they must move in separating, and the other perpendicular to the diameter, and productive of motion in the particles subjected to the stress, because its direction is the same that they would severally take in parting from each other. It is, indeed, most manifest, without the aid of rigorous mathematical demonstration, that no force, oblique to the direction pursued by the body to which it is applied, can operate with its full intensity upon that body, but must expend a portion of its energy upon some other object. Mr. Bakewell's error seems to me to consist in losing sight of this elementary truth, for, in using the semicircumference instead of the diameter in his estimate of the pressure at the extremities of the latter, he omits the resolution of the radial forces into their operative and inoperative elements, and assumes them to act with their full intensity to tear the cylinder asunder, notwithstanding the greater, or lesser, obliquity of every one of them, excepting the single central one perpendicular to the diameter. But, it may be said, what becomes of the other components of these radial forces, which act *parallel* to the diameter in question? Are they to be lost sight of, and do they produce no increase of strain upon the points

under consideration? for Mr. B.'s theory seems to be, that the cylinder does sustain from them a stress at the ends of the diameter, "in the disadvantageous manner of a string stretched horizontally, and bearing weight." They are certainly not to be forgotten, though as surely do they produce no such increase of strain. For, in the first place, it must be borne in mind that the body under consideration is a *cylinder*, every point of whose circumference is like all the rest, and that the two points in question, at the ends of any one of the infinite number of diameters, are supposed, for the time being, to be the *peculiar* seats of the stress, only for the sake of the argument and its illustration,—that all other points of the cylinder are precisely alike in their positions and functions. The central line of pressure just mentioned as perpendicular to the assumed diameter, and as producing the maximum of all the radial strains when they are referred to that diameter, is itself a part of a diameter which may, in its turn, become the subject of a similar assumption, and, when it does so, becomes the line of *least* (as it was before of *greatest*) pressure—that is, the line of no pressure at all. In the second place, the *parallel* unite with the *perpendicular* components of the radial forces to give the cylinder the *stiffness*, the supposed want of which is a main difficulty with Mr. B. It is the uniform tension of the steam which gives the required rigidity to the cylindrical vessel, however flexible its material; and if it were of gold leaf, or gossamer, it would still be perfectly stiff for the purpose in view, for its shape would continue *unaltered up to the moment of rupture;* and this preservation of form is all that absolute inflexibility in the shell, *per se*, could accomplish.

I have thus far argued the case upon the simple elementary principles which seem to me to govern it. I might proceed to give an analytical, or geometrical, solution of it, accompanied by diagrams; but as this would extend my communication beyond desirable limits, I will refer, instead, to the brief but satisfactory demonstration of Professor Barlow, in the part of his work upon the "Strength of Materials," under the head of "Strength of hydraulic presses and water pipes," (pages 205 to 210, London edition.) This well known treatise is probably accessible to most of your professional readers. It will be seen therein that the learned Professor upholds the received rule, and makes no mention of the existence of a doubt of its accuracy, among scientific men. It is true, he shows that, in consequence of the difference between the interior and exterior diameters of every hollow cylinder, the strain is not uniform throughout the thickness of the shell, but diminishes regularly from the inside to the outside thereof—so that, by the law "ut tensio sic vis," the metal being unequally strained in different parts of its section, the full resistance of all the fibres is not brought into action at the same moment; so that a deduction from the *actual*, in the calculation of the *effective*, thickness, is due to this cause. But the correction, on this account, for cylinders of large diameter, is so trifling, that it need not be considered in the present controversy. Thus, where r represents the (internal) radius, and t the actual thickness of the shell of the cylinder, the *effective*

thickness is expressed by $t \times \frac{r}{r+t}$; which, in the case of a boiler of 132 inches diameter, like the Medora's, would make the effective only 0.0061 of an inch (or about 2½ per cent.) less than the actual thickness, the latter being one-fourth of an inch.

The example by which Mr. B. illustrates his views of the theory of strength in a cylinder, although ingenious, seems to me inapplicable to the case, because it is inconsistent with the essential characteristic of the vessel, viz., its equality of resistance everywhere, resulting from the circularity of its form. It is not then allowable, even in argument, to suppose it to be cut at any point, and the cohesion (thus destroyed) of the material composing the shell to be represented by bolts acting as ties, unless we suppose those ties to take hold, not, according to Mr. B.'s supposition, of only two, or four, or more, insulated points, but of *every point of the periphery;* in which case, the argument drawn from the want of *stiffness* would fail, and the vessel would be, in effect, restored to its pristine condition of an undivided hollow body, equally strong everywhere; so that the reasoning would be as circular as the cylinder, and would come back by as roundabout a course to its point of departure.

It is necessary to use these hypothetical modes of reasoning with caution; nevertheless, I will offer an illustrative instance, which, I think, will make more clear the correctness of the positions already established upon abstract principles. Suppose that, instead of steam within the cylinder, (which, as Mr. B. has done, I will imagine to be a ring of an inch in width, and, consequently, giving a square inch for every inch in the length of its periphery,) we should introduce a solid disk of some hard body, fitting close against the interior of the ring. Let us then divide this disk into two equal parts, by cutting it through, on one of its diameters; let us further introduce a wedge between the halves, and force them apart by a power equal to that of steam of any given pressure, operating on the number of square inches contained in the length of the diameter—or, better still, let us introduce steam itself into the fissure, so that the two sections of the plate are forced asunder in directions perpendicular to the line of separation. The space between them need not be more than a hair's breadth to permit the full action of the fluid. Consequently, its pressure upon the ring at the ends of the opening will be on a mere line an inch long, and will produce no appreciable strain at those places. The only tension felt thereat will then be perpendicular to the line of the opening, and strictly tangential to the curve, and will be expressed by the number of inches in the diameter multiplied by the steam pressure per square inch. The ring might, indeed, give way indifferently at any point of its circumference, were it not that it has, at all other points than those at which the disk is cut, the benefit of its *friction* upon the latter. Will it now be contended that the strain upon the cylinder would be increased if the disk were removed, and the office it performs (of preserving the shape of the ring) be effected by the equal radial pressures of the steam?

Such are the conditions of the question as it presents itself to my mind; and they seem to me to consist with the established mode of computing the strength of cylindrical vessels, and with that only. As the elementary facts and principles on which such questions depend are often, however, far from being so obvious as to make the same impressions upon every mind, but, on the contrary, are frequently of so subtle and recondite a character as to elude the grasp of the inquirer, and are perhaps so in this instance, I would not dogmatically aver that the preceding exposition of the subject is sound beyond the possibility of a flaw. I would be glad that the matter should meet the consideration of others competent to its investigation. The question is a most important one, and has not, perhaps, been sufficiently discussed; at least, I do not at this moment recollect any elementary work in which it is treated of, except the brief notice of it by Professor Barlow, above quoted. It is time it should be finally settled by all the arguments and authorities which may be brought to bear upon it. If the rule heretofore used has indeed given results so much too favorable to the strength of steam boilers as Mr. Bakewell supposes, then the risks from which a watchful Providence has so often preserved those who have to do with them, have been far greater than we have been aware of; and while we feel increased thankfulness for past protection from these perils, we should not be unmindful to guard against them more effectually hereafter.

I have above considered only the effects, upon a hollow cylinder, of the *radial* stress of the confined fluid resolved into two resultants perpendicular to, and acting at the ends of, the diameter. The longitudinal strain parallel to the axis of the vessel has not been taken into view, for it is not clear that it, in fact, augments the tensile effect of the radial strain. It is not necessary, however, to discuss this question at present, and I therefore waive it.

In conclusion, permit me, gentlemen, (in reference to the catastrophe which led to this discussion,) to express the pleasure with which I perused, in your last number, the corrected calculations of the stress and strength of the boiler of the Medora, contained in an article prepared from particulars lately furnished by Mr. Reeder, who rebuilt the boiler after its explosion, and whose opportunities of acquaintance with all the details of its structure were much more perfect than my own, and were judiciously availed of by him, and their results lucidly presented by yourselves. BENJ. H. LATROBE, Civ. Eng.

Baltimore, May 18, 1843.

Remarks on the Explosion of the Boiler of the Steamboat Mohegan, on Long Island Sound. By THOMAS EWBANK, *Esq.*

To the Committee on Publications:

GENTLEMEN—An explosion occurred in one of the boilers of the steamboat "Mohegan," on the 24th ultimo. It was so slightly noticed by the daily press, and so trivial a character given to it, that no manifestation of feeling disturbed the public mind. At first I supposed

it hardly worth while to inquire into the particulars, or to trouble you with them; but, subsequently, taking up a paper which (after briefly noticing the affair) naively remarked that it was only occasioned by "a few sheets" having "slipped off the outside of the boiler," it became evident that the rupture was no trifling one. If a very small rent in a boiler be often sufficient to spread desolation over every part of a vessel, how much more the sudden displacement of whole sheets! Upon visiting the boat and examining the boiler, the conviction was irresistible, that, but for a fortuitous and apparently insignificant circumstance, this explosion would, in all probability, have been the most fatal on record.

Through the politeness of C. O. Handy, Esq., (President of the company by whom the boat is owned,) I am enabled to communicate every essential fact connected with the explosion, and also two strips from the sheet in which the rupture commenced. Let me premise the account with a few remarks, which, if they have no particular reference to the case of the Mohegan, have at least been elicited by circumstances connected with the present investigation.

It is gratifying to observe the jealousy and opposition dying away with which inquiries into explosions were used to be met, on the part of owners and officers. Ere long those gentlemen will, it is believed, generally acknowledge that such researches are of real value to themselves, not only as pointing out the true causes of explosions, the means of preventing them, and the enormous losses consequent on them, but as also contributing to remove doubt and suspicion from the public mind. People can have no confidence in traveling by steam while these disasters are so sedulously hushed up—while the causes of them are pronounced too mysterious to be meddled with—too occult to be explained.

As long as such sentiments are cultivated, people will naturally, and reasonably too, consider steamers as little else than floating volcanoes; and the boldest travelers will have secret misgivings when confined to the vicinity of boilers. Where there is uncertainty, there must be suspicion, or fear; but let the origin of explosions be made known—let people be convinced that there is no more mystery in the rupture of boilers, than in the breaking of a carriage axle, or the oversetting of a wagon, and that they may as certainly be avoided, and all dread of traveling by steam will become dissipated like a morning cloud.

Steam is destined to be the grand physical agent of man—to supersede animal and human drudgery, in a great measure, throughout the whole earth. Every person is therefore interested in its extension—the social, civil, and mental improvements of our race, are bound up with this plastic and powerful agent—all people in coming ages will be influenced, nay, moulded, by it. As a motive fluid, its blessings will be felt (like those of the atmosphere) over every part of our planet. Away, then, with the contracted ideas of those who look upon inquiries into explosions as interfering with their peculiar professions—as prying into subjects exclusively their own. Nothing can retard the universal adoption of steam as a first mover, so much as

those unworthy prejudices. Had such objections been always listened to, where there are now a hundred steam engines and steam vessels, there would not have been ten. The plea that inquiry tends to inflame, instead of to allay people's fears, is supremely absurd. The reverse ever has been, and must be, the case, no matter what the subject of inquiry may be.

But it is said, "Why inquire into steamboat explosions after they are over? The evil is then done, and cannot be undone. Inquiry can only end in blaming somebody, and perhaps without cause." Certainly these disasters cannot be scrutinized until they have occurred, no more than a boiler can be examined before it be made; nor can the calamities attending explosions be recalled. But, by critical examinations, similar evils may be prevented in future, and this is the legitimate object of inquiry. As for tracing the blame to 'somebody,' that may or may not be; but it is quite a secondary matter. It is well known, however, that the blame is often, and always unjustly, fastened upon "something." In some explosions, we are told the fault was in the feed pump—in others, the material of the boilers was defective—in others the flues, or the stay bolts, had given way. In some, the water, the fire, or the steam, &c., was the cause; as if there were neglect, inadvertence, waywardness, or deception, in these. They are the faithful slaves of man, ever attentive to the intelligent hand that controls them; and should they be abused for this? abused for invariably obeying the mandates of him whose power compels their obedience?

A feed-pump, like every other machine, always acts as its overseer wills it; that is, as he keeps it in working order, or not. Aqueous vapor will not accumulate in a boiler unless compeled to do so, nor can the flue and shell be rent asunder of their own accord. Is it the fault of a pump that it ceases to work? of the vapor, that its tension dangerously increases? or of the metal, that its texture and condition cannot resist the pressure brought to bear upon it? An unfeeling man overworks his horse till the animal dies from exhaustion: what would be thought of him, if, to exculpate himself, he laid the blame on the load? So, a boiler is overloaded with steam, and destroyed, but few think of laying the blame anywhere but on itself; as if *it* was to blame, or was expected to give warning of its condition—to betray its sufferings, like an animal, ere it expired. True, sometimes a boiler does so; but too generally it gives up the ghost in one unexpected and convulsive shudder.

A passenger in the street is knocked down by a ladder borne by two laborers behind him: what would be his surprise to find his remonstrance met by an assertion that the cause of his distress was in the ladder, and not in those who bore it? A person stumbles in the dark over a bucket, or breaks his head against a low beam: would it be wise to indict the one, or to call down negative blessings on the other? Now, something like this has been the way by which the works of man's hands have been blamed for his own forgetfulness, or neglect. Every material and every machine act by invariable laws;

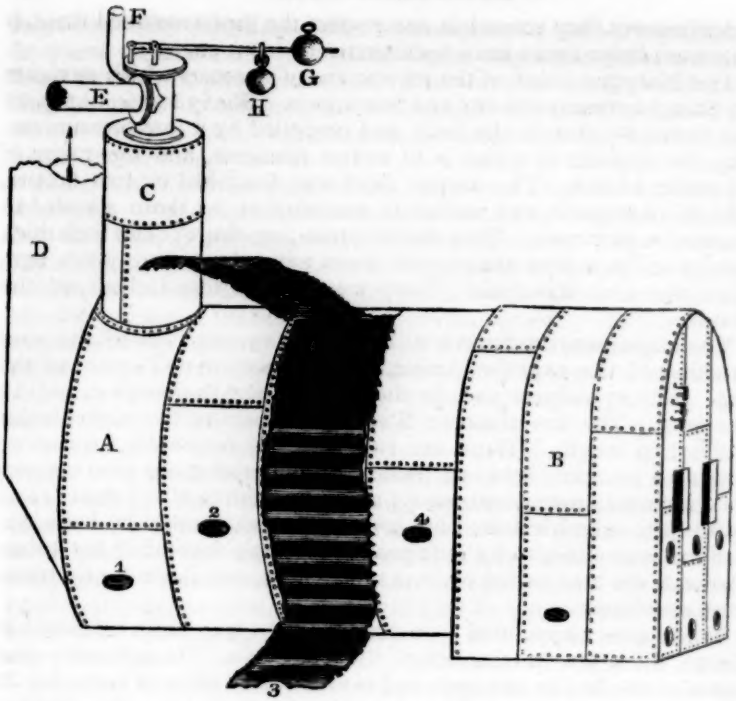
and whenever they exceed in any respect the limits assigned them, it is because those limits have been removed. But enough.

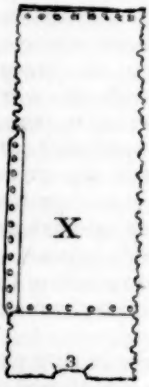
The Mohegan is one of the regular line of steamers which navigate the Sound between this city and Stonington. She is 180 feet long, 25 feet beam, 9½ feet in the hold, and propelled by a low pressure engine, the cylinder of which is 42 inches diameter, and the range of the piston 11 feet. The motive fluid was furnished by two boilers, alike in all respects, and similar in construction to those adopted in locomotive carriages. They are of copper, one-fourth of an inch thick nearly, or No. 4 wire gauge, and were new about four years ago, when the boat was built. They were placed fore and aft, on the guards.

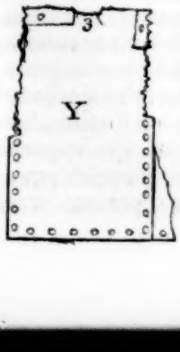
The accompanying figures will convey a correct idea of the construction of the exploded boiler, and the location and extent of the rent. The cylindrical part, or shell, A, is 7 feet in diameter, and 10 feet long. The fire chamber B is 6½ feet, making the entire boiler 16½ feet in length. There are two separate fireplaces, a water-leg forming a partition between them. The interior tubes (also copper) are 3 inches diameter, and occupy about two-thirds of the shell. The ends of the cylindrical shell are connected to each other by bolts riveted to them, and also by rods passing through several of the tubes. The ends are also united by bars to the sides, but there are no transverse stay-bars.

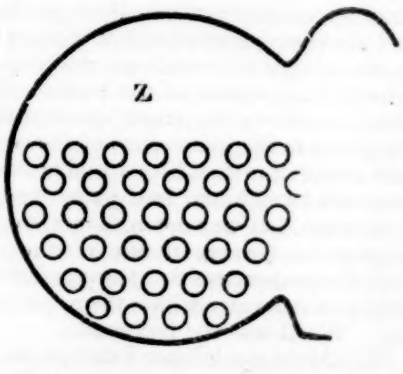
The engineer appointed soon after the boat was built, remained in charge till a few weeks before the explosion. He generally used steam of 25 lbs. to the inch, and is said rarely to have exceeded 30 lbs. The latter was considered within the limits of safety, though certainly verging towards those of danger. It exceeds the amount that a copper boiler of such a diameter ought to have been subjected to, according to the ordinary mode of calculation. From the comparatively large extent of fire surface, the evolution of steam was very rapid; and from the small space in the boilers for storing it, (small compared to the dimensions of the working cylinder,) particular care was required to prevent its undue accumulation. When the vessel was underweigh, the feed-pump was necessarily in almost constant requisition. It was not safe to allow it to remain many minutes unemployed. On account of excessive foaming, considerable experience with the boilers was necessary before a person could be certain of the height of the water within them. Oil was used to diminish the foaming. Wood was the fuel used.

The Mohegan left her berth in this city at 5, P. M., with 150 passengers. While passing through Hurlgate, the steam was turned off from the engine twice, in consequence of vessels crossing her path. The engineer states that he raised the safety valve on both occasions, and that it was while in the act of closing it, the explosion took place. Providentially, the rupture was in the starboard side of the starboard boiler, so that the contents, after blowing away the adjoining bulwarks, escaped overboard. Had the rent occurred on the opposite side of the boiler, the results must have been awfully appalling, as few of the passengers were below. Two or three persons were









A, cylindrical shell of boiler, showing the part ruptured, and a portion of the interior smoke pipes, or flues.
B, fire chamber. C, steam dome. D, chimney.
E, steam pipe. F, waste steam pipe and safety valve.
1, 2, 3, 4, elliptic hand-holes in the shell, at one of which (3) the rupture commenced.
G, stationary weight on the lever of the valve.
H, movable weight.
X, Y, ruptured sheets.
Z, transverse section of boiler at place of rupture.

slightly injured, but none seriously. Little injury was done to the boat, and none to the engine. The boiler was started by the recoil two feet, but was not overturned, nor the chimney pipe displaced. Both boilers have been removed, and will be replaced by a single one of iron, in which coal, instead of wood, will be used.

Several elliptic "hand-holes" were made in different parts of the boilers; there were four on each side of the cylindrical shell A, and arranged as seen in the figure. They are all of one size, viz., 6¾ by 4 inches. They were not originally formed in the boilers, but were made about two years ago, when the boat lay in Providence. Their edges are not strengthened in any way, but are plain and smooth, as the chisel that cut out the removed pieces, and the file, left them—an oversight, since the boilers were essentially weakened by them. Some precautions should have been taken to render the parts equally strong as before; a strong rim, or border, riveted round each hole, was necessary, as well to stiffen it, as to prevent the effects of checks, or notches, left by the chisel.

The utility of this will soon be apparent to the reader, if it be not already. The removal of the pieces cut from so large a copper shell, obviously rendered the borders of the openings incomparably weaker than any other part, and the interior covers made them still weaker, or, at least, had a tendency to do so. These covers lapped over them an inch, and were secured in the usual way; but it is obvious they transmitted to the borders of the openings the whole pressure to which they themselves were subjected. Thus, while the borders were less able to resist the force of steam than before the openings were made, a much greater one was accumulated through the covers upon them. Had the boilers been small cylinders of iron, the case would have been very different; but with comparatively soft and yielding copper, the action of the covers was not much unlike that of conical plugs, forced into openings to swell out their sides. Had the "hand-holes" been thus secured, the expense would have been trifling, and the boat might still have been running. With the exception of these "holes," with which the makers of the boilers had nothing to do, no defect whatever was to be found in their fabrication.

The rupture commenced near the bottom of A, at the hand-hole marked 3; this was in a line with 1. The rent was in the direction of the longer axis of the hole, separating the sheet, through its entire width, in nearly a straight line. The lower portion was blown outwards, and ripped from the adjoining sheets, on both sides, down to the deck; the upper one was torn, in like manner, from the neighboring seams, and thrown up to the roof of the boiler. See figures.

As the part of the boiler rent was cylindrical, of which no part was exposed to the fire, or to be otherwise deteriorated, it may be asked, What induced the rupture to commence where it did, in preference to any other place? Of course there was some cause for its beginning there? The circumstance alluded to at the close of the first paragraph, affords an answer to the question. A *crack* had long existed on each end of the hand-hole 3; one was an inch long, and the other rather more. These were noticed soon after the boat returned to the

city, and were distinguished from the parts newly rent by having fragments of red lead on them, and which had doubtless been squeezed in from the gasket of the cover. I had an opportunity of examining them, and have now a small fragment of copper exhibiting one side of the shorter crack.

Both cracks were probably made by the workman who formed the opening. In driving out the displaced piece of metal, after making an indentation round it with the chisel, it had become *torn* in those places to the extent named; and, from the exceeding brittleness of the copper, this is not surprising. They had been closed carefully up, and were not afterwards very observable, if at all. These cracks, then, or rather rents, determined the place of the rupture, and gave the first direction to it. And it was fortunate that they did, considering the position of the opening (3) with regard to the boat and deck. The rents, or cracks, acted the part of a safety valve, and gave to the rushing fluid the safest of all directions. Other cracks are still to be seen on the borders of the hole, (see the ruptured sheets X Y,) but whether they were there previous to the explosion, is not known.

A deficiency of water has been suggested as an approximate cause of this explosion, but without any facts, I think, upon which to base it. It is said that, when the engine was at work, no deficiency could be detected on account of the foaming; but when the steam was shut off the last time at Hurlgate, the water would immediately sink to its level, and leave the dome of the fireplace *bare;* the dome would instantly be heated to redness, and, when the foaming again covered it with water, on the renewed action of the engine, such dense volumes of vapor were suddenly generated, that the boiler was unable to confine them.

One conclusive objection to this hypothesis is—had the fire dome even been heated to incandescence, as supposed, it must have been, beyond comparison, the weakest part of the boiler, and must, while so heated, have given way. Another is, the captain and engineers declare that the engine was never wholly stopped at the gate—its speed was only slackened, and hence the foaming could never have entirely ceased. A third is, there is no indication of any part of the boiler having been injured by the fire.

Generally speaking, there is but one opinion respecting the immediate cause of this explosion, viz., an excessive accumulation of steam. This was probably only temporary, and occasioned by the partial stoppage of the boat immediately previous. The engineer asserts that there was no more than 20 lbs. of steam at the moment of rupture, and he is considered both cautious and intelligent. Though recently appointed to the Mohegan, he has been in the company's service several years. He usually carried steam of from 22 to 24 lbs. When the boat returned to the city, two weights were observed on the lever of the safety valve, (see figure,) one of which was reported to be an extra one; but this was not correct. Both weights had always been used—a stationary one, G, and the smaller one, H, used occasionally. Both were on the lever at the time of the explosion, the smaller one "about midway" between the end of the lever and the valve. In re-

ply to the question, What steam was required to raise the safety valve when the lever was loaded with one, or both, weights? the answer was—with both, 23 to 24 lbs.; with the stationary weight only, from 17 to 18 lbs.

With the permission of Mr. H., the following data for determining the force required to raise the valve with one or both weights, were procured. The diameter of the underside of the safety valves is six inches, giving an area of 28.2 inches. The lever is a bar of iron, half an inch thick; at one end, three inches deep, at the other, one and a half. The distance between the centre of the fulcrum and that of the valve, 4¾ inches. The weight G was stationed 54½ inches from the valve; it is a ball of iron, with an opening through its centre to slide over the lever, and a screw to fasten it in its place. Though not on the lever when examined, its place was strongly defined. It weighed 52 lbs. The weight H was suspended by a hook, or S, so as to be readily attached to any part of the lever; it weighed 28 lbs.

The pressure which the lever alone exerted on the valve, was 164 lbs. Then $52 \times 11\frac{1}{2}$ (the distance between the valve and G, divided by that between the valve and fulcrum,) + 164 = 762 lbs., the aggregate load on the valve with the stationary weight only. This, divided by 28.2, gives rising 27 lbs. on the inch; but add 180 lbs. for the weight H, and the force required to raise the valve could not have been less than 33 lbs., exclusive of friction, and that adhesion to which valves, it is well known, are subject.

The load on the valve, when not overcome by the steam, is, however, no criterion of the tension of the vapor at the instant of explosion. Of this, the gauge was the true and only test; and if the eye of the engineer was upon it immediately preceding the rupture, his statement that only 20 lbs. were on (if he was not mistaken) must be received. But the question then occurs—how is it that the shell of the boiler should have given way at 20 lbs., when, under the same circumstances, it has withstood steam at quite, or nearly, 40 lbs.? To this it has been replied—"The cracks where the rupture began had probably been strained, or started, and hence gave way with the lesser force." This is very unsatisfactory, for, if they had been strained, they would have leaked; but admit they did not—if steam, of an expansive force equivalent to 35 or 40 lbs. the inch, ever *started* the cracks at all, they must have been instantaneously rent wide asunder, for they were weaker when started than the moment before; and even a less force could then continue the rent than the one which commenced it.

It cannot, I think, be questioned, that part of the sheets which form the shell of the boiler, are of very inferior copper. The metal is so exceedingly brittle, that a single blow of a hammer has broken off portions as readily as if it had been bell-metal. Even pieces an inch square, on being laid over a hollow place, have snapped in two as if they had been spelter. Either the metal is deeply alloyed, or has been overheated in its manufacture, or else extremely hard rolled, though how the last operation could make its grain so short, is difficult to perceive. No responsibility could, however, in either case, be

attached to the builders of the boilers—the sheets, being merely bent into larger arcs, offered no opportunity to test the properties of the metal.

The specimens sent are from the sheet Y, and taken from the line of rupture. One piece is cut out in the direction in which the metal was rolled—i. e. the lengthways of the sheet—and the other in the opposite one. The places whence they were taken, are marked *o o*, on the upper part of Y. All the rents present the edges short and clean, almost as if cut with shears. The direction of the rents, with regard to the seams and rivets, is sufficiently obvious in the figures.

New York, May 1, 1843. T. E.

Statistics of fifty-four Passages (each way) of the "Great Western" across the Atlantic, between the years 1838 *and* 1842. *Time by the Chronometer.*

From Bristol, or Liverpool, to New York, viz., from Kingroad, or the Docks, to the Wharf.

Sailed.	Arrived.	Time.	Yearly av'ge time of trip.	Dist. run in naut. miles.	Passengers.	Yearly av. of passengers per trip.
		d. h.	d. h.			
April 8, 1838,	April 23, 1838,	15 10		3111	7	
June 2,	June 17,	14 16		3140	57	
July 21,	August 5,	14 18		3043	131	
September 8,	September 24,	16 9		3050	143	
October 27,	November 15,	19 0	16 1½	3100	107	89.
January 28, 1839,	February 16, 1839,	18 20		3114	104	
March 23,	April 14,	22 6		3350	110	
May 18,	May 31,	13 12		3086	107	
July 6,	July 22,	16 0		3030	114	
August 24,	September 10,	16 20		3025	113	
October 19,	November 2,	14 22	17 1½	3021	137	114.1
February 20, 1840,	March 7, 1840,	15 17		3058	77	
April 15,	May 3,	17 20		3093	100	
June 4,	June 18,	14 18		3073	85	
July 25,	August 9,	14 23		3018	97	
September 12,	September 27,	15 7		3049	54	
November 7,	November 24,	16 12	15 20	3025	40	75.3
April 8, 1841,	April 23, 1841,	15 12*		3096	44	
May 27,	June 10,	14 12		3053	42	
July 14,	July 29,	15 2		3014	98	
September 1,	September 16,	15 10		3036	111	
October 23, 1 15 P.M.	November 8, midnight,	16 12	15 9½	3035	127	84.2
April 2, 1842, 1 30 P.M.	April 17, noon,	15 4		3093	69	
May 21, 5 P.M.	June 4, 2 25 P.M.	14 2½		3020	64	
July 16, 1 P.M.	July 30,	14 1½		3028	65	
September 3, 5 P.M.	September 17, 10 P.M.	14 10		3020	97	
October 22, 3 P.M.	November 6, 6½ P.M.	15 8	14 15	3036	109	80.4
General averages,		15 20		3067	89	

* To anchor outside.

From New York to Bristol, or Liverpool, viz., from the Wharf to Kingroad, or the Docks.

Sailed.	Arrived.	Time.		Yearly av time of trip.		Dist. run in nautic'l miles.	Passeng's	Yearly av of pas'rs per trip.
		d.	h.	d.	h.			
May 7, 1838,	May 22, 1838,	14	0			3218	66	
June 25,	July 8,	12	14			3099	91	
August 16,	August 30,	13	2			3058	87	
October 4,	October 16,	12	12			3068	127	
November 23,	December 7,	13	16	13	4	3152	80	90.1
February 25, 1839,	March 12, 1839,	14	12			3133	36	
April 22,	May 7,	15	0			3352	113	
June 13,	June 26,	13	6			3033	115	
August 1,	August 13, 4 30 P.M.	12	10½			3067	64	
September 21,	October 4,	13	0			3034	43	
November 16,	November 30,	13	10	13	17	3038	31	67
March 19, 1840,	April 2, 1840,	14	4			3101	52	
May 9,	May 23,	14	2			3076	137	
July 1,	July 14,	13	12			3138	152	
August 18,	August 31,	13	1			3030	69	
October 10,	October 23,	13	6			3028	97	
December 9,	December 23,	14	9	13	21	3071	70	96.1
May 1, 1841,	May 14, 1841, 7 30 P.M.	13	1			3208	94	
June 19,	July 3,	14	2			3109	81	
August 7,	August 20,	12	10			3081	68	
September 25,	October 8,	12	13			3063	43	
November 23,	December 6,	13	5	13	1½	3049	30	62.4
April 28, 1842,	May 11, 1842, 4 A.M.	12	7½*			3248	77	
June 16, 2 30 P.M.	June 29, 7 30 A.M.	12	12			3225	99	
August 11, 2 15 P.M.	August 24, 2 P.M.	12	19			3106	70	
September 29, 2 P.M.	October 12, 11 P.M.	13	4			3048	35	
November 17,	November 30, 10 A.M.	12	15	12	16	3077	29	62
General averages,		13	9			3107	76	

* To light vessel.

Description of a Reflecting Lantern and a Heliotrope, used by Major J. D. GRAHAM, *as meridian marks for great distances, in* 1841, *while tracing, in his capacity of U. S. Commissioner, the due north line from the monument at the source of the river St. Croix.*

The lantern was constructed by Messrs. Henry N. Hooper & Co., of Boston, under Major G.'s directions. It was similar in form to the Parabolic Reflector Lantern, sometimes used in lighthouses, but much smaller, so as to be portable.

The burner was of the argand character, with a cylindrical wick, whose transverse section was half an inch in diameter, supplied with oil in the ordinary manner. This was placed in the focus of a parabolic reflector, or paraboloid, of sheet copper, lined inside with silver about one-twentieth of an inch in thickness, polished very smooth and bright. The dimensions were as follows:—

	Inches.
Diameter of the base of frustrum of reflector, - -	16.
Distance of vertex from base, - - - -	3.75
Distance of focus from vertex, - - - -	2.25
Diameter of cylindrical burner, - - - -	.50
Diameter of a larger burner, which was never used, but which, by an adapting piece, could be easily substituted, -	1.25

The instrument answered the purpose for which it was intended, admirably well, and was of great use in tracing the due north line. While it occupied the station at Park's Hill, 15 feet above the surface of the ground, or 828 feet above the sea, in the latter part of September, and early part of October, 1841, the light from it was distinctly seen with the naked eye, at night, when the weather was clear, from Blue Hill, whose summit, where crossed by the meridian line, is 1071 feet above the sea; the intervening country averaging about 500 feet above the sea, and the stations being 36 miles apart.

The light appeared to the naked eye, at that distance, as bright, and of about the same magnitude, as the planet Venus. Viewed through the transit telescope, of 43 inches focal length, it presented a luminous disk, of about thirty seconds of arc in diameter. From its brilliancy at that distance, Major G. has no doubt that it would have been visible to the naked eye at 50 miles, and through the telescope at 100 miles, could stations, free from interposing objects, have been found so far apart.

It was remarked, that the wick employed by Major G. was considerably smaller than that usually made, even for parlor lamps; and to this cause he attributed, in a great measure, the perfection with which the parallel rays were transmitted from the reflecting parabolic surface, so as to make them visible at so great a distance. Though a greater quantity of light is generated by a larger wick, the portion of rays reflected in a direction parallel to the axis, and which alone come to the eye, is the smaller as the flame transcends the focal limit. The size of wick most advantageous for use, may easily be determined by experiment: Major G.'s impression is, that, the smaller its transverse section, provided it is only large enough to escape being choked up by the charred particles, even one-third, or perhaps one-fourth, of an inch, the farther the light would be visible.

It has occurred to Major G. that lanterns of this description might be used with great advantage as station marks, in extensive trigonometrical surveys, requiring primary triangles of great length of sides. A revolving motion might be given to the lanterns, so as to make the light transmitted from them visible from many different stations within short intervals of time. Their simplicity, and the ease with which they are managed, would perhaps give them, for such purposes, a great advantage over the Drummond, or Bude, lights, even though they be not so brilliant as the latter.

The heliotrope, which he employed in the day time, was made by order of Mr. Hassler, at the instrument shop of the coast survey office. It was a rectangular parallelogram of good German plate glass, $1\frac{4}{5}$ by $1\frac{1}{3}$ inch in size, giving an area of reflecting surface of $2\frac{16}{100}$ square in. This also was seen at the distance of 36 miles. Proc. Am. Philos. Soc.

Practical & Theoretical Mechanics & Chemistry.

Notice of the Discovery of a new Metal by Mosander.

Extracted from Wöhler and Liebig's Annal. der Chim. und Pharm.

At the meeting of the Scandinavian Naturalists, at Stockholm, Mosander made a communication upon the subject of cerium and lanthanum. He exhibited, in the first place, some pale, clear, lemon-yellow oxide of cerium—described, generally, the characters of the protoxide and peroxide of cerium, and showed some of their salts. He then showed the colorless oxide of lanthanum, which remains colorless after ignition, and whose salts are also colorless. He then stated that the ores of yttrium, cerium and lanthanum, are accompanied, in the mineral kingdom, by a twin-brother, which is the cause of the known rose-red color of their salts, and which it is very difficult to separate from them, in consequence of their having the same solvents and precipitants. This body is the oxide of a hitherto unknown metal, to which Mosander has given the name didymium, (from the Greek διδυμος, a twin.)

The oxide of this metal is the cause of the brown color of the oxide of cerium, and of the brick-red of the oxide of lanthanum.

He also showed the dark brown oxide of didymium, and, for the sake of comparison, some crystalized salts of cerium, lanthanum, and didymium. The last are red.

He went on to say that he had not as yet succeeded in discovering a method for the perfect separation of these metals, and that this was the cause of the long delay of his communication on the subject of lanthanum; and that, had it not been for peculiar circumstances, he would not even yet have presented the results of his labors, which he could not consider but as imperfect. The preparations, he remarked, were not yet absolutely pure, but they were as nearly so as they could be made by spontaneous crystalizations of large masses of the solutions, in which the oxides had previously been purified as far as possible. A determination of the atomic weight of this metal, on account of the impurity of the preparations, he considered at present useless.

Notice of Walker's Hydraulic Elevator. By T. Ewbank.

To the Committee on Publications:

Gentlemen—I beg permission to reply to a remark of Mr. Wright, who, in the March number of the London Mechanics' Magazine, after paying a high, and, I fear, undeserved, compliment to my work on hydraulics, has inadvertently ascribed to it a defect which really does not belong to it, though doubtless it has many others. He observes—"There appears to be nothing on the subject of hydraulics that has escaped his researches, if we except a late invention of our countryman, Mr. John Walker, of Crooked Lane, London; the knowledge of which could not have crossed the Atlantic when Mr. Ewbank's book was published, or he would not have neglected to mention so novel and so simple an apparatus for raising water."

Mr. Walker's machine (described in the Journal of the Franklin Institute for August, 1842,) is the *Canne hydraulique* of French writers; three modifications of which Mr. Wright will find at pp. 372–3 of my book. Details of mechanism for *moving* hydraulic apparatus did not enter into my plan, and would have been incompatible with it; but had not the pages referred to passed the press ere Mr. Walker's "Elevator" was announced, his mode of combining two, or more, and of communicating motion to them, would have been noticed.

To the editor of the London Mechanics' Magazine, I feel peculiarly grateful for the flattering notice he has taken of my humble volume, in his popular and widely extended journal; and have no doubt that he will correct the unintentional error of his correspondent.

New York, April 27, 1843. THOS. EWBANK.

Cement Marble.

Translated for the Journal of the Franklin Institute, from the Journal des Chemins de Fer. By JOHN C. TRAUTWINE, Civ. Eng. and Architect.

An interesting paper on the cement marble of Mr. Keene, was recently read before the London Society of Arts.

This marble is a combination of sulphate of lime (gypsum) with alum. The gypsum undergoes the same preparation as the ordinary plaster of Paris, being deprived of its water of crystalization by heating it in a furnace. It is afterwards tempered with a saturated solution of alum, and, when this composition is again calcined and reduced to powder, it is ready for use. The cement is used like stucco; but, as it is susceptible of a high degree of polish, and may be colored by simply dissolving the proper mineral colors in the same water with which the powdered cement is mixed for the workman, we may thus obtain fine imitations of mosaics, and of veined marbles. This cement is not applicable to situations exposed to the influence of the weather, or of water; but it has been used in stucco for the interior of Windsor, Buckingham, and other, palaces. As it is of great hardness, it is well adapted to use as a facing instead of tiles; and it makes a good marquetry for paving. The interior of a church in London, and many other public buildings, have been paved in this manner.

FOR THE JOURNAL OF THE FRANKLIN INSTITUTE.

Digest of the Results of Experiments on Friction, performed by Capt. Morin, of the French Corps of Artillery. By Prof. J. F. FRAZER.

It has been thought advisable to digest into a tabular form, and publish in our journal, the results of a series of very elaborate and careful experiments, upon the subject of friction, performed at Metz, in the successive years from 1831 to 1834, under the sanction of the French government, by Capt. Arthur Morin, of the French corps of artillery.

These results have been more than once republished, and have been incorporated into several works upon practical mechanics; but we believe that no full statement of them has ever been made accessible to the American mechanic, and we have, therefore, carefully

tabulated them for our pages, believing that, in so doing, we are conferring no small favor upon the cause of our domestic arts.

In order to the more full appreciation of these results, it may not be useless to preface these tables by a very short sketch of the history of the study of this important mechanical agent.

The French philosopher, Amontons, appears to have been the first who undertook a regular series of experiments for the purpose of determining the value of friction; his results were first published in 1699. He arrived at the conclusion that friction was not affected by any increase or diminution of surface, but varied with the pressure imposed. He does not seem to have attempted any accurate valuation of this ratio for different substances, but states generally that the friction of one surface, sliding over another, is about one-third of the pressure, when the surfaces were clean, and that, when lubrics were used, it was the same for woods as for metals. He also concluded that it increased or diminished with the velocity, and varied in the compound ratio of the weight and pressure upon the rubbing parts, the length of time of their contact, and the velocity of their motion.

These views were adopted and advocated by De la Hire, and first questioned by Lambert. Parent proposed to reinvestigate the subject by means of the inclined plane, which plan was adopted and developed by the celebrated mathematician Euler. He supposed friction to be caused by the greater, or less, asperities of the two surfaces brought into actual contact, and, therefore, necessarily dependent upon the pressure; and assigned, as its practical value, one-third of the pressure, which was the conclusion of Amontons, but he doubted the effect of velocity upon it. We owe to him the beautiful demonstration of the fact, that, when a body rests upon an inclined plane, the angle of inclination of which is increased until the body begins to slide down, the ratio of the friction to the pressure is measured by the tangent of the angle of inclination; which fact furnishes a simple and easy method of determining this ratio.

Muschenbrock contended that friction increased with the surfaces. Bossut distinguished rolling from sliding friction, but erred in considering it as affected by time, but not proportional to the pressure, or mass. Desaguliers studied the subject attentively, and cites the experiments of Camus, which, however, were tried upon too small a scale to be satisfactory.

In 1779, seeing the vast importance of the full experimental development of the laws of this resistance, the celebrated philosopher Coulomb, at the request of the Academie des Sciences of Paris, undertook, at the arsenal at Rochfort, a very extensive series of experiments, which are, in fact, the first of any importance upon this subject, and of which the results are still very generally adopted by philosophers and mechanics.

He was led to the conclusion that, as a general law, other things being the same, the friction of the same surface, sliding over another, is proportional to the pressure with which the surfaces are urged together; but he thought that, in extreme cases, he found a deviation from this law; for that, when the pressures were extremely intense,

the friction did not increase in so great a proportion as the pressure. The deviation was, however, so inconsiderable, and happened only in such extreme cases, that it might, for the most part, be neglected.

The importance of this deduction is shown by the conclusion drawn from it, that, in the case of any body sliding upon another, the friction will be the same, whether the body move upon its face, or upon its edge. Thus, if we suppose a body to weigh 16 oz., and to present upon its face a surface of 16 square inches, while its edge has a surface of but one square inch, the friction of this body, while moving upon another, will still be the same, whether it be moving upon its face, or upon its edge. This law has, however, at all events, an evident limit, when its edge becomes sufficiently sharp to cut into the surface upon which it moves.

Another law which Coulomb deduced, and which has been confirmed by all subsequent experimenters, is, that friction is an uniformly retarding force, and is, consequently, independent of the velocity. The only exception to this, found by Mr. Rennie, is in the case of fibrous substances, such as cloths, very soft woods, &c.

Coulomb also came to the conclusions—

1. That friction varies with the quality of the surfaces, amounting sometimes, in the case of newly-planed woods, to half the pressure; while with hard woods, upon metals, it is only about one-fifth of the same.

2. That, as the surfaces are worn by attrition, the friction is generally diminished; but this has a limit, and the friction soon reaches a minimum. In woods, it was reduced by attrition from one-half to one-third of the pressure.

We shall see, directly, that M. Morin remarks upon this deduction, as pointing out the circumstances which led Coulomb into error in endeavoring to determine the friction between different substances.

3. That, between woods, the friction is less, when the grains cross each other, than when they are in the same direction, being in the former case one-fourth, in the latter one-half, of the pressure.

4. In general, friction is greater between surfaces of the same kind, than between those of different kinds.

This deduction, which has been so generally adopted since Coulomb's time, seems to be completely refuted by the experiments of Morin.

5. That friction diminishes as the smoothness of the surface is increased.

The experiments of Ximenes accord very closely with those of Coulomb.

Prof. Vince, (see Phil. Trans., 1785,) dissatisfied with the method in which these, and other experiments upon the same subject, were tried, resolved upon attempting to determine, by means of a different principle, whether the friction was solely dependent upon pressure. His method was highly ingenious, and apparently accurate, and his conclusion was, that the friction was not proportional to the pressure alone, but was also dependent upon the extent of surfaces in contact; so that a body, such as we have supposed above, would have less

friction when moving upon its edge, than when in motion upon its face. The experiments, however, were upon too small a scale to gain an implicit reliance.

The experiments of Mr. Rennie (Phil. Trans., 1829,) led him to results which, for the most part, confirm those of Coulomb. He found—

1. That the laws which govern the retardation of bodies gliding over each other, vary with the nature of the bodies.

2. That, with fibrous substances, (such as cloth, &c.,) friction is increased by surface and time, and diminished by pressure and velocity.

3. That, with harder substances, such as woods, metals, and stones, and within the limits of abrasion, the amount of friction is as the pressure, without regard to surface, time, or velocity.

4. That, with dissimilar substances sliding over each other, the measure of friction will be determined by the limit of abrasion of the softer substance.

5. That friction is greatest with soft, and least with hard, substances.

6. That the diminution of friction by unguents (lubrics) depends upon the nature of the unguent, without reference to that of the moving surfaces.

7. That the very soft woods, stones, and metals, approximate to the laws which govern fibrous substances.

The very great discrepancies thus shown to exist between the results obtained by the best experimenters, as well as the fact that the most extensive and valuable set of experiments (those of Coulomb) were tried chiefly upon materials used in naval constructions, induced Capt. Arthur Morin, in the year 1831, to undertake a series of investigations, which, it is not too high praise to say, for elegance of conception, accuracy of demonstration, and careful avoidance of all conceivable error, have not been paralleled in the history of this subject. These experiments were tried at Metz, and under the especial patronage of the French government, by whom he was provided with every facility for his undertaking.

The arrangements of M. Morin for accomplishing his object, we do not propose to describe in detail here, as they would require, for their comprehension, numerous and elaborate drawings; they may be seen, by those who desire it, by reference to his memoirs upon the subject, a copy of which is in the library of the Institute. The method which he adopted enabled him to conduct his experiments with much greater variations, both of surface and pressure, than had been formerly used; while, at the same time, the results were recorded by the apparatus itself with mathematical accuracy, and these records were preserved for future inspection and reference.

Under these circumstances, these investigations assume a peculiar value, and seem to justify our reliance upon their perfect accuracy. They will, therefore, we conceive, as they become fully known, entirely supersede all former experiments upon the same subject, and form, as Coulomb's and Rennie's have done, up to this time, the basis of all our calculations of this important resistance.

The following are the principal conclusions arrived at by M. Morin:

1. That, in all cases tried, notwithstanding the great variations of pressure, (from 200 to 6000 lbs.) the friction remained proportional to the pressure; so that the ratio, obtained by dividing the number expressive of the actual friction, by the pressure, remained always constant for the same substance.

We may observe that these experiments did not extend to those soft and fibrous tissues, such as cloth, &c., which were found by Vince and Rennie to obey another law.

2. That the friction is entirely independent of the velocity.

3. That it is entirely independent of the extent of surface.

These laws were found to hold good, even at pressures so great that the surfaces were materially abraded. M. Morin also satisfied himself that they governed the case of the friction of bodies during concussion.

Upon an examination of these tables, it will be found that the numbers expressive of the ratio of friction to pressure, determined by M. Morin, do not agree with those found by Coulomb, being in general considerably greater. In explanation of this difference, M. Morin adverts to the observation of Coulomb, that the surfaces used by him became more smooth and polished under attrition; which was never found by M. Morin to be the fact, when the surfaces, at the commencement of the experiment, were perfectly clean, and free from unguents. When, however, in the course of his experiments, a surface was accidentally used which had become unctuous in a former experiment, this phenomenon of the increasing polish was observed, and the ratio of the friction to the pressure was found to coincide very nearly with the number obtained by Coulomb. From this fact, M. Morin is led to the belief that the surfaces used by Coulomb were not prepared with sufficient care, but that they were, for the most part, more or less unctuous.

The observation made by M. Morin, that whenever two perfectly clean surfaces were moved in contact with each other, both were worn into alternate ridges and channels, accompanied by the formation of a powder, is not without great interest in the study of our subject.

This effect was produced invariably, whenever the pressures were tolerably great, and was frequently attended, in the experiments upon the woods, by the odor of burning wood.

The effect was found to be very great in the case of fibrous metals (such as wrought iron) sliding over each other; less in the case of granular metals (such as cast iron) sliding upon the fibrous; and least of all, when the granular metals were slid over each other.

M. Morin's experiments agree perfectly with those of Mr. Rennie in showing that, when lubrics are used, the ratio of the friction to the pressure is independent of the nature of the sliding surfaces. That, for instance, when lard is interposed between two surfaces of metal, the friction is that of lard upon lard, and not that of metal upon metal.

When the surfaces are wiped dry after the application of the lu-

bric, so as to be merely unctuous to the touch, the effect is intermediate between the two cases.

The conclusion come to by Coulomb, which has so completely passed into a general law of friction, that the friction between similar, is greater than that between dissimilar, substances, is not at all sanctioned by the experiments before us.

In reference to the influence of the duration of contact between the substances upon the friction, (by Coulomb called the friction of rest,) M. Morin shows very clearly its variation according to the nature of the substance experimented upon. When hard bodies rest upon each other, it reaches its maximum very quickly—with soft bodies, very slowly. When wood rested upon wood, the limit was attained in a few minutes—when the metals were in contact with wood, it required several days before the greatest effect was attained.

With these remarks, we submit the tabulated results of these experiments to our readers. It will be recollected that these tables present, as it were, a digest of the labors of M. Morin; every result thus recorded being a mean of a number, frequently of a very great number, of experiments, tried under every conceivable variety of circumstances, with large and small surfaces, under light and heavy pressures, with low and high velocities; the machine being so regulated as to cause, at one time, an accelerated—at another, a retarded—at another, an uniform—velocity. Of all these circumstances, the pressure alone was found to affect the friction; and the ratio of these (the substance remaining the same) varied within limits far less than those which may fairly be allowed in such experiments.

Table I, presents the results of the friction of substances sliding upon each other. In the first column is recorded the nature of the surface which was movable in the experiment, together with that of the surface at rest over which it slid. In the second column is indicated the direction of the fibres of the substances in relation to the direction of the motion, and to each other. The remainder contains the various numbers expressive of the constant ratio of the friction to the pressure. These are expressed by the decimal obtained by dividing the measure of the actual friction by the pressure which caused it. This portion of the table is divided into two compartments, one containing the results obtained during the motion; the other, those given when, after remaining in contact for a considerable length of time, the bodies were first caused to move over each other. Each of these compartments is again subdivided into columns, containing the results with different lubrics; the first recording those obtained when the surfaces were perfectly clean; the last (headed surface unctuous) recording those given when the lubric had been carefully wiped off, so as to leave the surface barely unctuous to the touch. With this explanation, we believe the table will be perfectly intelligible.

Table II is drawn up in the same general form, and contains the results of the experiments upon the friction of axles of different materials upon their bearings, and, among other important information, conclusively shows the advantages to be derived from attention to the method in which the lubric is supplied in such cases.

TABLE I—*Table showing the Result of Morin's Experiments on the Friction of Plane Surfaces, sliding over each other.*

Materials.		Direction of the Fibres.	Ratio of the Friction to the Pressure.																	
			During Motion.—Lubric.										After Contact.—Lubric.							
			None.	Water.	Dry soap.	Tallow	Lard.	Sweet oil.	Lard & pl'bago	Asphalte.	Cart grease.	Surface unctu's	None.	Water.	Dry soap.	Tallow	Lard.	Sweet oil.	Surface unctu's
Oak,	Sliding upon Oak.	Fibres parallel to the direction of the motion.	.478	.25	.164	.075	.067					.108	.625	.71	.44	.164			.390
"		Fibres of sliding pieces perpendicular to those of the sleepers, and to the motion.	.314										.54						
"		Fibres of both pieces at right angles to the direction of the motion.	.336			.083	.072					.145				.254			.314
"		Fibres of sliding pieces vertical—of sleepers parallel to the motion.	.192										.271						
Elm,		Fibres parallel to the direction of the motion.	.432		.137	.070	.060					.119	.694		.411	.142	.277		.420
"		Fibres of elm at right angles to those of the oak and to the motion.	.45										.57						
Ash,		Fibres parallel to the direction of the motion.	.40										.57						
Fir,		do.	.355										.52						
Beech,		do.	.36			.055						.153	.53				.330		.300
Wild Pear,		do.	.37										.44						
Service tree,		do.	.40										.57						
Wrought iron,		do.	.619	.256	.214	.085							.62	.649		.108			
Cast iron,		do.	.49	.218	.189	.078	.075	.075				.104		.646		.101	.103	.102	
Brass,		do.	.617			.069						.100	.62			.095			
Tanned leather		Fibres of oak parallel. Leather laid flat.	.296																
Curried "		" "	.265										.74						
Sole "		" "	.52										.605						
" "		" Leather on edge.	.335																
Hemp—bands,		Fibres parallel.	.52	.29								—	.43	.79					
" plait of small cords		"	.32										.64						
													.50						
Hemp in shreds		Threads perpendicular to the motion.		.332										.869					
" old rope 1½ in. diam		"	.52										.79						

Elm,	upon elm.	Fibres parallel.		.139									.140			.217					
Oak,		"	.246		.136	.073	.066						.136	.376			.178				
Cast iron,		"	.195			.077		.061	.091				.125								
Wrought iron,		"	.252			.078	.076	.053					.138								
Oak,	upon cast iron.	"	.372			.08							.168								
Elm,		"				.066						.098	.135								
Hornbeam,		"	.394			.07	.071	.068	.058	.06	.095		.136				.131	.136			
Lignum vitæ,		"				.076		.076					.121								
Wild Pear,		"	.436			.067	.068						.173								
Tanned leather		Leather flat.	.559	.365		.159		.133					.229		.621				.122		
" "		" on edge.		.338				.135				.267			.615				.127		
Cast iron,			.152	.314	.197	.100	.070	.064	.055				.144	.162			.100				
Wrought iron,			.194			.103	.076	.066				.124		.194			.101		.113		
Steel,			.202			.105	.081	.070					.109				.108				
Brass,			.189			.072	.068	.066				.134	.115						.103		
Bronze,			.217			.086		.077					.098				.106				
Hemp in shreds		Shreds perpendicular to the motion.						.153													
Oak,	upon w't iron.	Fibres parallel.				.194							.149								
Lignum vitæ,						.098		.072					.166								
Cast iron,						.098	.058	.063				.155	.143				.100		.100		
Wrought iron,			.138			.082	.081	.070					.177	.137			.115				
Steel,						.093	.076														
Bronze,			.161			.081		.077	.089				.166						.164	.172	
Lignum vitæ,	upon bronze.					.082		.053					.146								
Leather,		Leather laid flat.				.241		.191	Bronze wet with water				.287								
"		" on edge.				.138		.135					.244								
Cast iron,			.147			.085	.070	.067					.132								
Wrought iron,			.172			.103	.075	.078				.168	.160								
Steel,			.152			.056		.053	.067			.170									
Bronze,			.201					.058					.134								

TABLE II.—*Experiments upon the Friction of Axles upon their Bearings.*

		Ratio of Fric. to Pressure			
		Oil.	Lard	Tallow.	Uncts
Wrought or cast iron upon Cast iron or bronze	Lubric renewing itself	.075	.075	.075	.13 to .16
	" constantly renewed	.054	.054	.054	
Bronze on bronze	" renewing itself	.101		.093	
Bronze on cast iron	" constantly renewed	.052		.045	
Lignum vitæ on cast iron	" renewing itself		.116		.153
Lignum vitæ	" constantly renewed		.070		
Cast iron on Lignum vitæ	" "	.092		.092	.100
Wrought iron	" renewing itself	.114	.135		.118

A new Method of distinguishing between the Spots of Arsenic and Antimony obtained in the Apparatus of Marsh. *By* FRESENIUS.

Extracted from Wöhler and Liebig's Annal. der Chemie for September, 1842.

The description of the apparatus devised by Mr. Marsh for the detection of arsenic, in the case of suspected poisoning by a preparation of that metal, and the method of its use, will be found in full in a former number of our journal, (Jour. Fr. Inst. vol. xviii, p. 338, 2d ser.) The great liability to error with this apparatus, lies in the difficulty of distinguishing between the traces left by the two metals, arsenic and antimony, and the consequent doubt thrown on the results obtained. The following method, extracted from the Annalen der Chemie und Pharmacie, for September, 1842, seems to us the most simple and unexceptionable for the supplying of this want, and the perfection of the apparatus, as an agent in medico-legal inquiries.

Obtain marks, as distinct as possible, in the usual way with the apparatus, and proceed (changing occasionally the tubes) as long as spots are given.

Then remove the vessel in which the gas is generated, and substitute an apparatus for the generation of sulphuretted hydrogen, (being careful, of course, that the materials used are free from arsenic.) Pass the stream of this gas over the metallic spots, so slowly that the gas will just burn as it issues from the end of the tube, which is drawn out to a fine orifice.

While the gas is coming over, heat the tube by means of a spirit-lamp, beginning at the outer end of the apparatus, and passing the lamp slowly along in a direction contrary to the direction of the current of gas. By this means the metal, or metals, present are converted into sulphurets. When this conversion is perfect, remove the apparatus for the generation of sulphuretted hydrogen, and replace it by an apparatus for the generation of hydrochloric acid gas, interposing a larger tube, filled with cotton, between this apparatus and the tube in which the spots are contained. Let the stream of this gas pass

slowly along the tube, and the sulphuret of antimony, if any be present, will be dissolved instantaneously, if the spots be thin, and in a few seconds if they be thicker; for this substance is very volatile in an atmosphere of hydrochloric acid.

On the other hand, the sulphuret of arsenic, if any be present, will be unaffected, and, after all the antimony is removed, may be obtained by sealing the end of the tube, and filling it with aqua ammoniæ, by which it will be dissolved, and may be again deposited, by evaporation, in a watch-glass, or other convenient vessel, and may be subjected to any tests which the operator may think proper.

British and American Royal Mail Co.'s Steam Ship Hibernia.

We have great pleasure in noticing this magnificent steamer. On going on board of her at the Broomielaw, we were first shown into the main saloon, and were as much delighted by its elegant appearance, as we were struck by its great size. It is constructed on a plan introduced by Mr. Napier into the first Halifax steamers, being built upon the main deck, and extending from the mainmast to the taffrail, ample room being allowed on each side of it for conveniently working the vessel; the stanchions, or frames for its support, pass through the main deck to the second deck, and, being firmly fixed to both, render it perfectly secure.

On descending the first flight of stairs into the engine-room, a gallery, chiefly of malleable iron, which passes entirely round the engines, gives an excellent view of the whole machinery. The next flight leads to the starting stage, which is of open cast-iron work, and fills up the space between the engines. The last flight leads to the lower floor and boilers. The cylinders are 77½ inches diameter, and 7 feet 6 inches stroke. The starting gear is very beautiful and effective; a large malleable iron wheel, highly polished, with appropriate handles, is keyed into a shaft, having on it a pinion which takes into a sector on the valve-shaft, enabling three men to work the large D valves easily. The handle for disengaging the eccentric rod throws the pinion into gear with the sector, as soon as the eccentric rod has cleared the pin, so that the starting wheel can never be driven round by the motion of the valve-shaft. The throttle-valve, injection-cock, and blow-through-valve-handles, are all within reach of the engineer at the starting-wheel, as is also the handle for throwing the expansion valve in or out of gear.

The counter, for registering the number of strokes, is worked by the air-pump-crosshead of the starboard engine, being placed in the arch of the framing above the air-pump. Opposite to it, on the corresponding arch of the larboard engine, is placed the clock. From this counter may be told at a glance the number of strokes that the engines have made, from the time of the vessel's leaving England, until her return from America. The side-levers are much shorter, in proportion to the length of the stroke, than is generally the case, and on this account the engines take up much less room. The levers are made, in this instance, of just sufficient length to allow of the air-

pump, condenser, and valve-casing, being introduced, without being at all crowded, and giving the engineers plenty of space for the performance of the necessary operations about them. The great length of the side-rods fully justifies this shortness of lever, they being more than 1½ times the length of stroke, and the half-versed sine of the arc described by the lever being only about four inches, causes the side-rods to work very slightly off the perpendicular, which would be but very little diminished by increasing the length of the lever to the standard of general practice. Considering the great weight of the side-levers, and the immense power by which they are urged, it might be expected that the main centre brasses would speedily wear out; on the contrary, however, the experience derived from other engines of Mr. Napier's construction, warrants us in considering them almost indestructible; for, after many years' use in those of them which we have examined, the wear is almost imperceptible, proving incontrovertibly that the friction on the main centre is scarcely anything.

The valve-casings are fitted with expansion joints, having brass stuffing-box covers. The air-pumps are of brass, as are also the buckets; the rods are cased with brass, which passes a considerable distance into the eye of the bucket; the end of the rod being also sheathed, effectually preventing any galvanic action taking place. There are two discharge-valves, one on the top of the barrel of the pump, and the other in the hot-well, and also a stop-valve in the discharge-pipe. The four bilge-pumps are placed under the midship-levers, all of which, as well as the plungers, are of brass; the suction and discharge-pipes are all of copper. The blow-through-escape-valve has its pipe passing under the air-pump into the condenser, which is an improvement upon the usual plan of attaching it to the bottom of the air-pump, as, in case of anything getting into it and preventing its shutting, the air-pump is not thereby prevented from doing its work. The other plan has been the cause of many breakages. The feed-pumps are placed according to Mr. Napier's usual plan, above the wing-side levers, or the hot-well, from which they are supplied, and into which the superfluous feed-water is returned.

There are four distinct boilers, having four fires in each. The flues do not pass over the top of the fires, but have two tiers at the back end. Two of the boilers are fired from the fore, and two from the after, part of the vessel. All four are connected by stop-valves to a general receiver, or steam-chest, from which the steam passes by two distinct malleable iron pipes under the first platform to the valve-casings. The boilers and steam-pipes are felted, and the cylinders are felted and cased with wood, which causes the engine-room to be as comfortable as any part of the vessel. Each of the boilers is fitted with an apparatus by which the brine is pumped out. This, in its passage, is made to give out part of its heat to the feed-water. At the after-end of the boilers is a malleable-iron stair, for the greater convenience of the firemen, to the coal-boxes and deck. The fore and after holds and coal-boxes above the boilers, and alongside the engines, will contain coals for 24 or 30 days' consumption.

The principal dimensions are as follows—Length of keel and fore-

rake, 218 feet; depth of hold, 24 feet; breadth of beam, 36 feet; diameter of paddles, 30 feet 4 inches. Glas. Prac. Mech. & Eng. Mag.*

Notice of an Altitude and Azimuth Instrument, presented to the Royal Astronomical Society by Admiral Greig. Made by M. Reichenbach, of Munich.

The diameter of the azimuth circle is 15, and of the altitude circle 12, inches. The divisions, which are of silver, read to 4″ of space by 4 verniers upon each circle. The instrument is one of the kind which admits of repetition both in the horizontal and vertical planes, and is furnished with two telescopes; the principal one resting in Ys attached to the azimuth index, and the other placed below the azimuth circle, according to the ordinary arrangement. But a peculiarity deserving especial notice, is the manner in which the usual difficulty of observing near the zenith is obviated. A diagonal reflector, in this case a prism, directs the rays through one of the pivots of the transit axis, in which the diaphragm and eye-piece are consequently placed, and thus the observer remains in an easy, unaltered position, whatever may be the altitude of the object observed. It is almost superfluous to add, that the graduation axes, tangent screws, and other delicate parts of this instrument, exhibit all the proofs of care and skill for which the maker was so long celebrated. Lond. & Edinb. Philos. Mag.

Method of distinguishing Zinc from Manganese, in Solutions containing Ammoniacal Salts. By M. Otto.

If solutions of chloride of zinc and chloride of manganese, containing much hydrochlorate of ammonia, be rendered alkaline by solution of ammonia, the addition of the smallest quantity of solution of hydrosulphuric acid precipitates white hydrated sulphuret of zinc, whilst no effect is produced by it in the solution of manganese, more being required to obtain a precipitate of the sulphuret of the latter metal. If acetic acid be then added to the solutions, the sulphuret of manganese dissolves very readily, whilst that of zinc remains undissolved. M. Otto advises the use of hydrosulphuric acid, and not hydrosulphate of ammonia, because the latter, always containing persulphuret, may occasion mistakes, since acetic acid separates sulphur from it. If, for example, it be required to determine whether iron filings contain brass, they are to be dissolved in aqua regia, the peroxide of iron is to be precipitated by ammonia, the liquor is then to be acidulated, the copper precipitated by hydrosulphuric acid, and ammonia is then to be added to the filtered liquor, which usually still contains a sufficient quantity of hydrosulphuric acid. If a white precipitate be formed, which does not dissolve in acetic acid, it shows that zinc is present. M. Wackenroder has especially recommended the solubility of sulphuret of manganese in acetic acid, to separate manganese from other metals.—*Jour. de Pharm. et de Chem.*, Sept. 1842. Ibid.

* From this excellent work we extracted the article on the crank, in our last number, but accidentally omitted the proper credit.

METEOROLOGICAL OBSERVATIONS FOR MARCH, 1843.

Moon	Days.	Therm. Sun Rise.	Therm. 2 P.M.	Barometr. Sun Rise.	Barometr. 2 P.M.	Wind. Direction.	Wind. Force.	Water Fallen in rain	State of the Weather, and Remarks.	
⊗	1	28°	30°	29.75	29.75	W.	Moderate		Cloudy.	Cloudy.
	2	16	22	29.83	30.00	W.	Brisk		Clear.	Clear.
	3	14	21	30.10	30.10	NW.	Moderate		Clear.	Clear.
	4	16	27	30.10	30.05	NW.	do		Clear.	Clear.
	5	18	30	29.90	29.85	NW.	Brisk		Clear.	Hazy.
	6	18	32	29.90	29.90	W.	do		Clear.	Clear.
	7	19	32	30.00	30.10	NW.	do		Clear.	Clear.
	8	17	41	30.05	30.00	NW.	Moderate		Clear.	Hazy.
☾	9	31	45	30.00	30.05	W.	do		Clear.	Light clouds.
	10	33	36	29.96	29.85	E.	do	.53	Snow.	Rain.
	11	34	41	29.57	29.75	W.	Brisk		Cloudy.	Clear.
	12	36	40	30.10	30.10	NE. E.	Moderate		Clear.	Par. cloudy.
	13	32	33	29.84	29.60	NE.	do	.65	Sleet.	Rain.
	14	17	28	29.85	29.95	W.	Brisk		Clear.	Clear.
☉	15	35	42	29.66	29.66	SW.	Blustering		Cloudy.	Flying clouds.
	16	28	30	29.94	29.80	NE.	Brisk	1.22	Cloudy.	Snow.
	17	26	29	29.40	29.43	NW.	Blustering		Cloudy.	Cloudy.
	18	19	29	29.30	29.54	W.	Moderate		Clear.	Clear.
	19	18	30	29.64	29.64	W.	do		Clear.	Clear.
	20	16	37	29.70	29.73	W.	do		Clear.	Hazy.
	21	20	33	29.86	29.86	W.	do		Clear.	Clear.
☽	22	22	43	29.74	29.65	SE.	Calm	.01	Par. cloudy.	Flurry of snow.
	23	18	22	29.50	29.50	W.	Blustering		Clear.	Clear.
	24	15	25	29.66	29.74	W.	Moderate		Clear.	Clear.
	25	20	43	29.84	29.76	SW.	Calm		Clear.	Lightly cloudy.
	26	20	31	30.00	30.14	W.	Brisk		Clear.	Clear.
	27	26	37	30.26	30.16	E. S.	Moderate		Cloudy.	Snow.
	28	46	52	29.40	30.10	S. S.	do	2.48	Rain.	Rain.
	29	31	40	29.10	29.90	W.	Brisk		Clear.	Clear.
⊗	30	32	52	30.10	30.10	SE.	Calm		Cloudy.	Clear.
	31	37	39	30.10	30.00	E.	Moderate	.35	Cloudy.	Rain.
		24.45	34.90	29.81	29.86			5.24		

Thermometer. Maximum 52 on 28th & 30th. Minimum 14 on 3d. Mean 32.175

Barometer. Max. 30.26 on 27th. Min. 29.10 on 29th. Mean 29.285

APRIL, 1843.

Moon	Days.	Therm. Sun Rise.	Therm. 2 P.M.	Barometr. Sun Rise.	Barometr. 2 P.M.	Wind. Direction.	Wind. Force.	Water Fallen in rain	State of the Weather, and Remarks.	
	1	35°	43°	29.60	29.70	NE. W.	Moderate	.15	Cloudy.	Rain.
	2	26	42	30.05	30.10	NW.	do		Clear.	Par. cloudy.
	3	32	47	30.15	30.16	NW. W.	do		Cloudy.	Clear.
	4	34	46	30.10	30.10	E.	do		Cloudy.	Cloudy.
	5	32	40	29.94	29.90	NE.	do	.15	Snow.	Cloudy.
☾	6	33	47	29.80	29.80	W.	do		Clear.	Clear.
	7	37	48	29.90	29.70	W.	Brisk		Clear.	Clear.
	8	40	60	29.40	29.40	SW.	Moderate		Cloudy.	Cloudy.
	9	40	53	29.40	29.36	W.	do		Par. cloudy.	Par. cloudy.
	10	36	46	29.60	29.65	W.	Blustering		Clear.	Flying clouds.
	11	33	55	29.86	29.86	W.	Brisk		Clear.	Clear.
	12	37	66	29.90	29.94	W.	Moderate		Clear.	Clear.
	13	47	50	30.00	30.00	E.	do		Cloudy.	Rain.
☉	14	51	54	29.54	29 86	E.	do	1.40	Rain.	Rain.
	15	63	62	29.80	29 80	SE.	do		Cloudy.	Cloudy.
	16	52	71	29.87	29 87	NW. SE.	do	.17	Fog.	Rain.
	17	55	60	29.75	29.75	S. SE.	do		Cloudy.	Cloudy.
	18	42	42	29.90	29.90	NE.	Brisk	.30	Rain.	Cloudy.
	19	38	43	30.00	30.00	E.	Moderate		Cloudy.	Cloudy.
	20	40	60	30.03	30.00	N.	do		Clear.	Cloudy.
☽	21	43	63	30.05	30.05	W.	do		Clear.	Clear.
	22	47	72	29.94	29 94	S.	do	.37	Cloudy.	Rain.
	23	54	63	29.76	29.65	S.	do	.27	Drizzle.	Shower.
	24	53	64	29.50	29.50	W.	do	.07	Clear.	Showery.
	25	53	72	29.70	29.76	W.	do		Clear.	Flying clouds.
	26	55	70	29.78	29.40	E.	do		Par. cloudy.	Par. cloudy.
	27	52	73	29.60	29.40	N.	do		Par. cloudy.	Par. cloudy.
	28	45	70	29.80	29.80	W.	Brisk		Clear.	Clear.
⊗	29	55	78	29.85	29.85	W.	Moderate		Clear.	Clear.
	30	48	57	29.80	29.75	E. SE.	do		Cloudy.	Cloudy.
		43.60	56.57	29.81	29.81			2.88		

Thermometer. Max. 78. on 29th. Min. 26. on 2d. Mean, 50.085

Barometer. Max. 30.16 on 3d. Min. 29.40 on 8th 9th 26th 27th. Mean 29.81

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